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A SCIENTIFIC QUARTERLY DEALING WITH EXTRASENSORY PERCEPTION, THE PSYCHOKINETIC EFFECT, AND RELATED TOPICS

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The Journal *of* Parapsychology

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AN INVESTIGATION OF ESP IN FIRST AND SECOND GRADES IN AMERICAN SCHOOLS[1]

By J. G. VAN BUSSCHBACH

ABSTRACT: This research on a possible parapsychical relationship between teacher and pupil was a continuation of former work conducted by the same experimenter in Holland and in the United States. The subjects in the new research were first- and second-grade pupils in the public and private elementary schools for white children in Durham, N. C. The procedure varied somewhat from the one used in previous tests with the same grades in Holland. In brief, the pupils tried to guess pictures, symbols, and colors being looked at by the teacher, who was concealed from view.

The first half of the experiment gave positive scores; the second half, negative, so that the overall scores were not significant, as they had been in the Dutch work. There was, however, a significant difference between the two halves. During the first weeks of testing, heavy snows closed the schools from time to time. Later, the lost days had to be made up on Saturdays and holidays, and it was during this make-up period that the negative scoring took place. The author thinks it likely that unconscious frustrations caused by having school on the normally-free days were the reason for the decline in scoring.—Ed.

INTRODUCTION

THE investigations described here concern the extrasensory relationship between teachers and pupils of the first two grades of the elementary schools in Durham, North Carolina.

The test procedure was, generally speaking, the same as that followed in former schoolroom tests for ESP reported by the writer

[1] This research was done while the writer was a Visiting Research Fellow in the Parapsychology Laboratory of Duke University, February-July, 1960.

The data of this experiment were left on file at the Parapsychology Laboratory when Mr. van Busschbach returned to the Netherlands. The results and analyses have been completely rechecked by a member of the Laboratory staff, and the editors take the responsibility for the accuracy of the figures.—Ed.

(1, 2, 3, 4, 5): the teacher, acting as "sender" or agent, is seated back of a screen at the rear of the class. She focuses her attention silently upon a set of cards in random order, one after another. The pupils (subjects) are asked to guess the cards. The procedure is directed by a test leader, sometimes with the aid of an assistant. Only the teacher has any sensory awareness of the target for each trial.

In this connection, it may be good to point out again that the basic purpose of all my school ESP tests is to make a study of the relationship between the educator and the pupil as a part of the relationships between people in a general sense. Accent has been placed upon the occurrence of ESP as one factor that might influence the normal teacher-pupil relationship.

Education consists of transferring objects of consciousness from educator to pupil. Extensive studies exist about the way in which this occurs, the factors at work in it, and the conditions which influence the transfer. Up through the eighteenth century, the principles of pedagogy were speculative and its practice was largely intuitive. Rationalism brought changes. The development of psychology provided a better basis of information, and the nineteenth century brought education as a practice under the influence of associationism and intellectualism. The educator-pupil relationship was analyzed in a more concrete way. Intuition, considered an unreliable and unmeasurable factor, was neglected and could not hold a place in the new, rational system of education. Thus the possibility that intuition had any role in the transfer of educational ideas and influences was denied.

Nevertheless, persistent and widespread experiences from everyday life suggested the occurrence of ESP, and parapsychology has today accumulated conclusive evidence in support of this factor. The pedagogical field has become an important area for parapsychological research—one which has proved to be a source of new data and hypotheses for further experimentation and conclusions. Pedagogy certainly needs the help of parapsychology to grow to full maturity. On the other hand, it is important for parapsychology to operate in combination with other branches of science in order to come out of its more or less isolated position. These studies of

ESP in the schoolroom situation thus continue to serve a valuable purpose that should be mutually beneficial to parapsychology and educational psychology.

The present investigation was planned as a sequel to tests which had been conducted with first- and second-grade pupils in Dutch schools (5). It is necessary, therefore, to recall the conditions and results of that work as forming the essential background for the presentation and interpretation of the present experiment. The test materials were selected as appropriate for subjects of the first- and second-grade level. Three different kinds of target materials were employed, each being used for a series (run) of 12 trials, and the range of choice on each trial was limited to three possibilities instead of the usual five. The first run was based upon a random order of geometrical objects (triangle, square, and circle); the second, common objects (tree, house, and boat); and the third, colors (red, yellow, and blue). The pupils were not required to write their responses, but were given answer forms in which they simply had to cross out their choice among the three objects on each trial, which were printed in different orders from trial to trial.

Classes were selected on the basis of the interest of the teachers in participating in the program. Two series of tests were made during the Dutch experiment, one in the city of Amsterdam and the other in Dordrecht. In each city, a different test leader had charge of the testing. There was one difference in the objective testing procedure: in Amsterdam, the subject's response sheet was shielded so that only the three targets from which the choice was to be made on each trial were visible, through the slit in the outside cover, and the pupil had to slide the record sheet up after each trial to bring the next set of targets into view; in the Dordrecht work, all 12 choice possibilities were visible during the entire run.

The results of the Amsterdam series were, generally speaking, within the range of chance variation, while the tests made in Dordrecht were highly significant. When the results of the two cities were pooled to give a general total of all the Dutch work in the first two grades, the overall scores were still highly significant, with odds against chance of approximately a million to one.

Procedure

The study with which this paper is concerned consisted of tests made in all but one of the first and second grades of the public and private elementary schools for white children in Durham, N. C. There were four different test-leaders: Miss Margaret Anderson of the University of Pittsburgh, Mrs. Ruth McRacken, Director of Elementary Education in Durham, and Mrs. Frances Greene and Mrs. Peggy Murphy, staff members of the Parapsychology Laboratory.[2] During each test, two of these assistants were in the classroom, one of them directing the test itself and working with the students and the other staying in the back of the classroom during the test and assisting the teacher.

While the tests were based generally upon the same procedure as was used in the Dutch investigations in the same grades, there were differences in details. These were partly planned beforehand and partly unexpected, unavoidable, and unwelcome. As mentioned previously, all the former tests were done in selected classes in which good discipline and a good atmosphere generally prevailed and in which the teachers involved were interested in the test and were thus able to co-operate fully from both the practical and psychological points of view. This time it had been planned that *all* first- and second-grade classes in the white Durham schools would be tested. Thus, in this study, we worked with some classes in which the situation was less favorable. Formerly the explanation of the test was given by the test leader (the person in charge of the testing), but this time we asked the teacher to explain the test to her pupils in order to accentuate her central position in the test and to emphasize for the pupils that the test leader's role was secondary to the teacher's. The teacher was permitted to depart from the written instructions, and the uniformity in the explanation of the test was lost.

A further change from the former procedure was an experimental safeguard introduced into the handling of the target cards so that no one taking part in the experiment except the teacher-

[2] I wish to express my appreciation to these four persons who contributed so generously of their time and efforts during the investigation, and I wish also to thank the many others who helped through discussions, advice, encouragement, analyses and evaluation of data, and in other ways.

agent had the opportunity to see the order of the targets. Formerly, the random order list, unsealed, was given to the teacher by one of the investigators. In the present experiment, the order was made up[3] by an assistant on the laboratory staff, and it was carefully shielded from the test leaders. This assistant put the lists of random digits in a sealed opaque envelope which was not opened by the agent before the beginning of the series in which they were to be used. Each series had its own target list and each one was used just once.

The target materials, the record sheets, and the method of recording choices were the same as had been used in the Dutch work with first and second grades. The record sheet was handled by the pupils in such a way that a whole column for recording 12 trials was visible at one time, as had been done in the Dordrecht series. The order of using the three series of targets was regularly changed from session to session so that each group of targets (geometrical forms, objects, colors) occurred the same number of times in the first, second, or third place in the session, whereas they had always been used in the same order in the Dutch investigation.

In the tests in the first and second grades in the Netherlands no prizes were awarded. In Durham we began without prizes; but after 34 classes, Mrs. Murphy suggested making cardboard "crowns" on which was written "Best Guesser" in gold letters. This was put in front of the class during the test, and the winner of the class got the crown.

During the test period there was abnormally bad weather, with such heavy snowfall that the schools could not operate on the regular schedule. For this reason, between March 2 and March 23 appointments for conducting tests had to be cancelled nine times and the tests postponed. Since Mrs. Greene and Mrs. Murphy had not had experience in testing elementary school classes before, Miss Margaret Anderson, who was experienced in this area of research, came from Pittsburgh to assist in the first 12 classes. But after the first day of her visit, the schools were closed by the snow and her assistance had to be limited to four classes.

[3] From *Tables of Random Sampling Numbers* by M. C. Kendall and B. Babington Smith, 1954.

During the period March 25 and April 18, the school days lost because of the snow and other weather circumstances had to be made up. For this purpose, the schools were obliged to operate on the normally-free Saturdays and on the normally-free Easter Monday. There were many objections to this requirement from the teachers as well as from the parents, not to speak of the pupils. Since we had lost so many days already in the test period, we could not stop testing between March 25 and April 19, and we had to accept the consequences, but we did not test on Saturdays and on Easter Monday.

Results

This investigation included 54 different classes from 13 schools. There were 27 first grades with 711 students, and 27 second grades with 697 students. Altogether, 1,408 unselected pupil-subjects and 52 different unselected teacher-agents were used. (There were two cases of combined first and second grades under the same teacher.) Each of these 1,408 subjects did 36 guesses, with a total of 50,482 guesses (206 trials not being counted because of no response or multiple responses), which makes this investigation the largest ESP

Table 1

General Results, Separated According To Grade and Sex of Subjects

	No. of Subjects	No. of Calls	Hits	Dev.	CR	Av. Scoring (%)
First-Grade Boys	367	13117	4382	+ 9.67	+ .18	33.4
Second-Grade Boys	361	12946	4332	+16.67	+ .31	33.5
Total Boys	728	26063	8714	+26.34	+ .35	33.4
First-Grade Girls	344	12345	4101	−14	− .27	33.2
Second-Grade Girls	336	12074	4089	+64.33	+1.24	33.9
Total Girls	680	24419	8190	+50.33	+ .68	33.5
Total Boys & Girls First Grade	711	25462	8483	− 4.33	− .06	33.3
Total Boys & Girls Second Grade	697	25020	8421	+81	+1.09	33.7
Grand Total	1408	50482	16904	+76.67	+ .72	33.5

study ever done as a unit in schools. The total hits were 16,904. This represents a positive deviation of 76.67, which yields an insignificant CR of .72. Table 1 gives these overall results, divided, in accordance with previous practice, into first and second grades, boys and girls. None of the groupings of the data in Table 1 is significant. It is clear, therefore, that the results did not provide a direct confirmation of the earlier results when the data were analyzed along lines followed in previous studies. But there were different trends in scoring at different stages of the research which were apparent to the investigators as the work progressed. It therefore seems important to examine the data on an exploratory basis to see whether there is unexpected, suggestive evidence that ESP was occurring, and if so, to gain as much insight as possible into the factors which were most likely affecting it in such a way as to obscure the psi function in the total data.

Table 2

GENERAL RESULTS FOR THE FIRST AND SECOND HALVES OF THE EXPERIMENT PRESENTED CHRONOLOGICALLY ACCORDING TO SCHOOL

School	No. of Subjects	No. of Calls	Hits	Dev.	CR	Av. Scoring (%)
1	93	3318	1137	+ 31.01	+1.14	34.3
2	117	4201	1427	+ 26.67	+ .88	34
3	47	1678	582	+ 22.67	+1.17	34.7
4	17	612	221	+ 17	+1.46	36.1
5	35	1254	402	− 16	− .96	32.1
6	184	6617	2240	+ 34.32	+ .90	33.9
7	83	2967	1013	+ 24	+ .93	34.14
8	126	4518	1549	+ 43	+1.36	34.3
Total 1st Half	702	25165	8571	+182.67	+2.44	34.06
9	156	5595	1838	− 27	− .79	32.9
10	156	5608	1815	− 54.34	−1.58	32.4
11a	60	2150	666	− 50.66	−2.32	31
12	133	4754	1575	− 9.67	− .30	33.1
11b	61	2189	740	+ 10.33	+ .47	33.8
13	140	5021	1699	+ 25.34	+ .76	33.8
Total 2nd Half	706	25317	8333	−106	−1.43	32.9

CR_d (1st half—2nd half) = 2.73
P = .0063

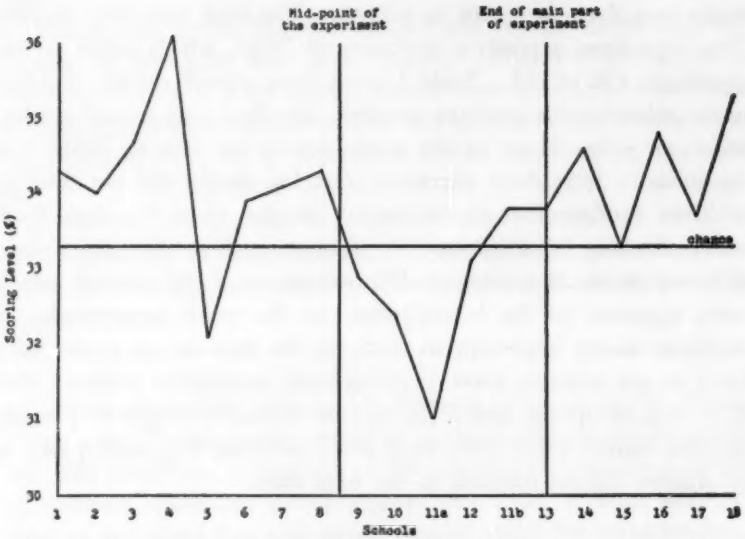

FIG. 1. Average scoring rate for each school in the chronological order of testing.

The scoring started out with a positive trend. This level of performance was above mean chance expectation in the first four schools and generally so in the first half of the experiment. These results may be seen in Table 2 and in Fig. 1, where the results are shown in terms of the average scoring rate for each school in the chronological order of testing.

At the end of the first half of the planned work (making the most nearly equal division that is possible without splitting any class), the results were 8,571 hits out of 25,165 trials. This represents a positive deviation of 182.67 hits and a CR of 2.44 ($P = .015$). Of the 28 classes which contributed these results, 19 scored above the chance level, one at expectation, and eight below. As Fig. 1 indicates, seven of the eight schools (which accounted for 26 of the classes tested during this period) were in the chance or above category, while one school (with only two classes) scored negatively.

The second half of the experiment gave 8,333 hits in 25,317 trials. This gives a negative deviation of -106 and a CR of -1.43 ($P = .15$). It is evident from Table 2 and Fig. 1 that the scoring

was consistently negative for the first part of the second half of the investigation and that it then turned once more toward a positive trend. The first four schools tested during the second half of the series provided a total of 21 classes, only 7 of which showed a positive deviation, while 14 were negative. (Table 2 and Fig. 1 show that the testing in school No. 11 was done in two sections, one before the work in school No. 12 and one after.) The last five classes all came from one school, three giving positive total scores and two negative, with the school as a whole being positive.

When a statistical test is applied to the difference between the two halves of the experiment in order to get a quantitative measure of the reversal of scoring trend, the CR of the difference is found to be 2.73 ($P = .0063$). This evaluation is based upon the binomial hypothesis and does not take account of the fact that the test procedure involved multiple calling of the target orders. It is necessary, therefore, to make some allowance for the change in the evaluation that might be expected if the Greville method had been applied. Repeated uses of this method on such data in the past have shown that the binomial CR is reduced by a small amount, generally less than 10%. A 10% correction applied in this instance reduces the CR to 2.46 with a P-value of .014. Thus there is some reason for assuming that ESP was operating in the tests, and closer scrutiny of the conditions may provide valuable clues to the psychological factors affecting psi.

After finishing the work with the first and second grades as planned, we tested four third grades and repeated the test in one first grade. The reason for doing these will be taken up in the discussion section. These extra classes are included in Fig. 1 since they are interesting in relation to the chronological development of the experiment and they tend to reinforce the impression, already stated, that there was a return to consistent above-chance ESP performance at the end of the series. However, these extra tests were not a part of the experimental plan and the results are not included in this report except as shown incidentally in Fig. 1.

The results of the boys and girls were analyzed separately, school by school, in order to see whether there was any influence of sex on the distribution of the ESP effect. The chronological curves of

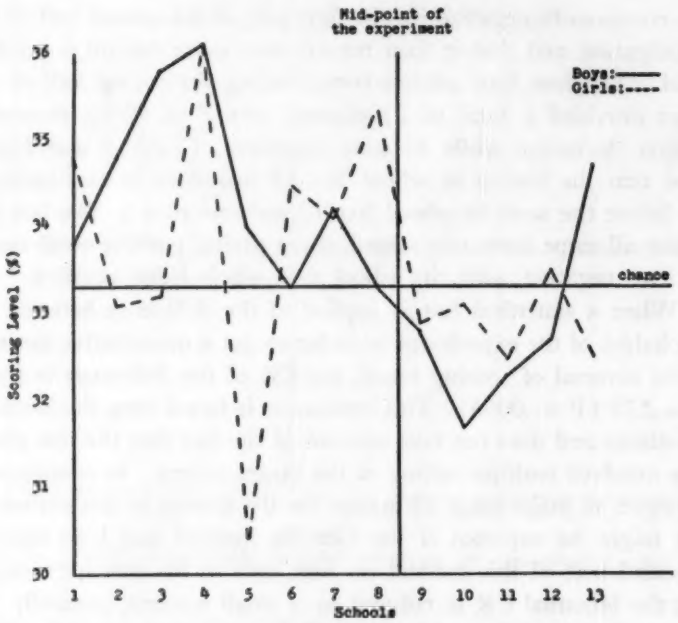

FIG. 2. Average scoring rate for boys and for girls in each school.

score averages for boys and girls in successive schools are shown in Fig. 2. Inspection of these two curves suggests that the differences in the scores between the initial, positive- and the following, negative-scoring period was larger for the boys than for the girls. While the total results of the boys and of the girls did not differ significantly, in some cases there were striking differences between boys and girls in individual classes, especially in the first grades, the highest CR of the difference out of 13 schools being 2.56. A chi-square test of the significance of these differences (squaring and adding the CR's of the difference between boys and girls by grades for separate schools) yields 21.69 (13 d.f., $P = .07$) for the first grades and 16.03 (12 d.f., $P = .19$) for the second grades. (One school did not have a second grade.) All classes thus give $\chi^2 = 37.71$ (25 d.f., $P = .05$) for the over-all differences between the boys and girls considered in terms of each grade in each of the schools tested. These statistical results only reach a suggestive level

of significance, but to this extent they support the impression given from the curves in Fig. 2 that sex may have been a factor in the results. If it is assumed that this was the case, the trend (the boys contributing more of the ESP effect) was opposite to that found in the first two grades of Dutch schools.

The difference between the total results of first and second grades was not significant (CR's = − .06 and 1.09, respectively, the second grades being very slightly better than the first grades). This is in contrast to the results from the same grades in the Netherlands, in which first grades scored a CR of 5.34 and the second grades a CR of 1.13, with the CR of the difference = 2.68.

No difference worthy of notice was found among the three kinds of target material. Nor was there any evidence of position effects among the three runs or series within the session. The previous Dutch work with the first two grades similarly failed to reveal any difference in this respect, but in that experiment the three kinds of materials had always been used in the same order within the session.

Discussion

On the basis of previous work in the schoolroom with the same basic procedure, the general expectation at the outset of this investigation was that the total results would be statistically significant. This expectation, as has been pointed out, was not realized. From the point of view of independent confirmatory evidence of the occurrence of ESP in the teacher-pupil relation, the results are inconclusive. But the preceding work in this area has already provided therefore justified in examining the results of this investigation on enough evidence that ESP occurs in the schoolroom, and we are an exploratory basis on the general assumption that ESP might have been operating in ways that did not yield a significant total score. In exploring the results for possible scoring trends, therefore, we will not be attempting to reach conclusions, but will be looking for possible factors that will require further supporting evidence from new research before they can be fully accepted.

The fact that the most interesting results of this investigation are in the area of unexpected differences serves to underscore the fact that the psi function in normal situations and with large numbers of subjects presents a very complicated problem. Many com-

plex details will have to be studied over a long series of experiments before we can hope to have an adequate view of the extrasensory process in the psychical contact between educator and pupil.

The difference between the first and second halves of the experiment might at first seem like another instance of the widespread "decline" in psi performance. But there are reasons why this classification should not be applied to the present results without looking more closely into the circumstances. First, most declines have been found in connection with smaller units of the test, such as the run, set, or page. So-called series declines or chronological declines have been much less common. Second, this result cannot be compared with declines in selected subjects, which are usually referred to as "loss of ability to score in the psi test." Here we are dealing with different subjects in each session, and any loss of psi performance due to changes in attitude as the experiment progressed would have to be traced to those in charge of the tests and not to the subjects. Third, there was not a decline merely to the line of mean chance expectation, but to a level of negative scoring. Thus the unfavorable period may have involved psi-missing, and not merely a small *amount* of ESP function. Finally, there are indications that the performance returned to the former (and usual) above-chance level when the conditions again became favorable.

The difference in the results at different stages, especially that between schools 9-12 and the others, is so significant that it is justifiable to look for the most likely explanation. Since there was no change in the test personnel, the assistants were perfectly accustomed to their task during the negative-scoring period and were even relaxed by the short vacation provided by the snows. The atmosphere of the classes was good, and the co-operation of the teachers in the second half of the series did not show any difference of attitude or interest from those of the other schools. The most likely factor may be an unconscious resentment and frustration in the classroom atmosphere during the period in which the schools were forced to operate on the normally free days between March 25 and April 19. The CR of the difference between this period and the rest of the time is 3.10, this difference being contributed primarily by the boys ($CR_d = 2.93$).

The atmosphere in the classroom is a very subtle factor in the relation between teacher and pupil as well as in its influence upon the pupil's parapsychical abilities. This atmosphere does not always depend on direct factors—for example, good discipline and good instruction—but very often on external influences which work on the pupil's mind, consciously or unconsciously. Unusual events in the recent past as well as in the immediate future may disturb the normal situation. For example, no experienced teacher likes to give important test work during the week before Christmas vacation, for he knows that the anticipation of this event is working long before in a frustrating way upon the results of his pupils.

The fact that the first grades scored somewhat lower than the second in Durham while they were significantly higher in the Netherlands might be due to a variation in the test situation that was not planned and was not encountered before. In the Netherlands more than 90% of the first-grade pupils attend a kindergarten school for one to three years before starting in the first grade. In Durham, only a small percentage have this preschool experience. The consequence is that the first-grade pupils in the Netherlands are much more used to classroom procedures and more practiced in working with figures and symbols such as the ones used in the tests. They are also accustomed to playing in the same general way as the ESP tests were done so that the kind of game offered by the test is familiar to them. The system of education in the Dutch infant schools (kindergartens) is based on the play-work principle, and this cultivates a particular atmosphere in the classes.

The reason why we tested also in the third grades was to get an idea whether or not the increase of the results between the first and second years might be projected into the successive grades. Lack of time prevented our finishing this part of the investigation. Only four third grades were tested altogether, so we could not draw any conclusion about the relation between the second and third grades. All sessions gave positive deviations, as did the session in which the first grade with the highest score was retested. Since this extension of the original plan covered the period after the lost days had been made up, the return to positive scoring gives some assurance that our impressions regarding the unfavorable factors were

correct. At the same time, however, there was no indication that *higher* scores were made in these four third grades nor in the repeated first grade.

This report does not pretend to offer any conclusions. It has aimed at presenting the essential facts and examining them for clues to factors that appear to deserve serious study in further investigations along these lines. The results strengthen an impression already formed on the basis of my previous work with GESP tests in the teacher-pupil relationship. This is the impression that the factor being studied is very elusive, and its detection depends upon conditions that are difficult to identify, define, and control. In the Dutch work with first and second grades, for example, only chance results were obtained in Amsterdam while highly significant results were obtained in Dordrecht. While one can point to differences between those two series which might have been responsible for the contrast in the findings, it is not possible to say definitely at this stage what the explanation is. The present experiment is the first one of my own investigations in which any of the tests were characterized by appreciable below-chance scoring. This reversal of direction of success or psi-missing, already familiar from other types of ESP investigation (1), is a further indication of the fact that the function with which ESP tests are concerned is essentially psychological and unconscious in nature. At the present stage of our knowledge, its demonstration in a test situation, even in the familiar relationship between teacher and pupils, is not to be taken for granted.

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THREE EXPLORATORY STUDIES OF ESP IN A GAME SITUATION[1]

By RENA J. RATTE

ABSTRACT: A clairvoyance test put into the framework of a competitive game procedure was the basis of three ESP studies of an exploratory nature. The game, called Psi-Chek, was played by two people (the experimenter and a subject) using a pack of ESP cards, and a game board on which to tally game points. Half of the time the players' objective was to win by guessing the ESP cards correctly, and half, to win by avoiding the correct cards. In all three series the total scores were negative when low-aim was the objective, and positive when high-aim was the objective. In the first and third series, the difference between the two categories was statistically significant; in the second, it was not. An analysis, made on the basis of the results for individual subjects, suggested that the ability to score high or low at will was a fairly general one, though this analysis did not reach a level of statistical significance with the limited number of subjects used. —Ed.

INTRODUCTION

THE DEVELOPMENT of good ESP games—that is, games which are enjoyable and which at the same time allow for adequate controls against sensory cues—is difficult. Experimenters in the past who have worked along these lines have done so in the hope that the use of a game procedure would prevent boredom and fatigue in the subject and thereby improve scoring. In 1946-47, Birge combined an ESP clairvoyance technique with a game procedure involving the use of poker chips and small prizes for high scores (1), but his results gave only suggestive evidence of ESP. Later, in 1949-50, Steen carried out a gamelike experiment with dice called "PK Baseball" which was designed to capitalize on the widespread interest in baseball while testing a complex PK hypothesis (7). Although the test yielded significant results, the game procedure did not prevent a decline in scoring as the experimenter had hoped it would. In the years since these researches, there has been a large amount of ESP

[1] This study was conducted at the Parapsychology Laboratory of Duke University. The work was supported by a Ralph Drake Perry Fellowship. I should like to thank Mrs. Peggy Murphy and Miss Nancy Fullmer of the Laboratory staff for assistance in checking the data, and Mrs. Sarah Bilberry for serving so faithfully as a subject.

work with school children. Although these tests have not usually involved game procedures as such, the experimenters have in most instances believed that favorable motivation was enhanced by the creation of an imaginative, gamelike setting.

Recently, a program was initiated at the Duke Parapsychology Laboratory for the further development and use of game tests. As the first results, Frances Greene and I reported an experiment in September, 1960, in which Steen's baseball idea had been worked into a technique called "PK Basketball" (4). The subjects, counselors at a girls' camp, played the game for recreation. The experimental procedure produced significant results that supported the PK hypothesis. While there was no basis of comparison to indicate how successful these same subjects would have been in a non-game PK test, there was some indication of a connection between the subjects' scores and their feeling of being lucky or unlucky as judged by a questionnaire worked out by Mrs. Greene (2).

Another exploratory investigation (3) was aimed at studying more explicitly the effects of the game procedure and of competition as such in the test. The results gave some indications that both elements favored PK performance, and again there was a suggestion that PK success was associated with a feeling of being lucky in general.

Procedure and Results

The three series of ESP trials here reported were based on a new game, Psi-Chek, developed by Mrs. Greene and Mrs. Peggy Murphy in the Duke Parapsychology Laboratory. In the second and third series, there were progressive attempts to modify the game so as to make it a more adequate testing tool and, at the same time, to retain as many as possible of the game elements incorporated in the design.

In these studies the game was played by two people (the subject and the experimenter, R.J.R.) who competed with each other for high game score. Psi-Chek was played in two ways: originally the purpose was psi-missing (trying *not* to hit the target); but in this investigation, the procedure was adapted, in half the runs, to score for psi-hitting. For psi-missing, each of the players has a pack of ESP cards, shuffled and placed face-down in front of him. He

does not know their order. He also has a game board having five vertical columns and ten horizontal rows. Each of the ten rows on the board is given a value in terms of game points. The first row at the bottom has a −10 value; the second, 0; the third, 10; the fourth, 20; the fifth, 30; the sixth, 40; the seventh, 50; the eighth, 70; the ninth, 100; and the tenth, 150. The player is given five game markers, each with one of the five ESP symbols painted on it. At the beginning of the game, the subject places each of his markers in one of the five vertical columns at the second, or 0-value, row.

For this first turn he chooses a marker and moves it up one space from the 0 line. He tries to choose one with a symbol which will *not* match the top card on his opponent's face-down pack of ESP cards. (Since the opponent himself does not know what that card is, this is a clairvoyance test.) If the marker and card do not agree, the trial is a success and the marker stays where it is. If they do agree, the marker must be taken down to the −10 line. Then the opponent carries out the same procedure for his turn, and so on through the 25 cards of the deck. If the player at any time fails (hits the target card) no matter how far up the column he has been able to move a marker, it must go back to the 0-row. The object is to get a high game score by getting a low ESP score.

In the three series included in this report, the game was also played backward; that is, for psi-hitting. In this case, the players first place their markers on the highest (150-value) row. On each playing turn (trial) they attempt to move the one with the symbol which *does* appear on the target card. With each call, the game marker selected is moved one row down the board. If the call is correct (the called symbol matches that on the target card) then the marker is returned to the top row. Otherwise it retains its lower position. The game ends when each player has had 25 turns. The aim of the game played this way is to finish with the highest game score possible by getting a high ESP score.

Series I

In the first series, 13 unselected subjects were used. Seven were university students, one was an adult visitor to the Laboratory, and five were Laboratory members.

The ESP targets used were "open" packs made by an assistant from a random number table. A fresh pack was used for each run.

The series was planned to consist of 100 runs of 25 calls in which the subject attempted to miss the target, and 100 runs in which he attempted to hit the target. In each testing session, the subject played the psi-missing game twice through for a total of 50 calls, and also played the psi-hitting game twice through for a total of 50 calls. The two games of one kind were played consecutively, and the order in which the types of game were played was alternated so that neither type held a preferred temporal position in the study as a whole.

The conditions of the first study were entirely exploratory. The cards used were unscreened. The experimenter recorded both the call and the target card in the presence of the subject, although the record sheet itself was screened from the subject's sight. When the subject made his call for the experimenter's top card, the experimenter turned up only as much of the target card as was necessary for her to see the symbol. If the symbol was a hit, the card was shown to the subject, but not otherwise. The subject's target card was then recorded. The experimenter then made her call for the top card on the pack in front of the subject, and the subject turned up the card so that both players could see it, whether or not it was a hit. (The experimenter's calls were not included in the data evaluated in this report for any of the three studies.)

The results of this exploratory study seemed promising. (See Table 1.) In the 2500 psi-missing trials, the subjects made 464 hits, with a deviation of -36. In the 2500 psi-hitting trials, the subjects made 547 hits, with a deviation of $+47$ ($CR = 2.35$; $P = .019$). The CR of the difference was 2.93 ($P = .003$).

As a matter of general interest, the data were checked for forward displacement. It was found that the forward displacement showed the same trend as the direct hitting. In the 2400 psi-missing forward displacement trials, there were 469 hits, giving a deviation of -11, and in the same number of psi-hitting forward displacement trials, there were 520 hits giving a deviation of $+40$ ($CR = 2.04$; $P = .04$). The difference between the scores was not significant.

Table 1

SUMMARY OF RESULTS: SERIES I, II, AND III

	DIRECT HITS			FORWARD DISPLACEMENT	
	Psi-Missing	Psi-Hitting	CR_d	Psi-Missing	Psi-Hitting
SERIES I					
Trials	2500	2500	—	2400	2400
Hits	464	547	—	469	520
Dev.	−36	+47	2.93	−11	+40
CR	1.80	2.35	—	—	2.04
P	—	.019	.003	—	.04
SERIES II					
Trials	2500	2500	—	2400	2400
Hits	467	524	—	466	491
Dev.	−33	+24	2.02	−14	+11
CR	1.65	1.20	—	—	—
P	—	—	.04	—	—
SERIES III					
Trials	2500	2500	—	2400	2400
Hits	472	554	—	502	507
Dev.	−28	+54	2.90	+22	+27
CR	1.40	2.70	—	—	—
P	—	.0069	.0037	—	—

No analysis could be done for backward displacement since the subjects knew on each trial whether they had succeeded or failed at hitting the target. This, of course, would allow them to produce a backward displacement effect through ordinary means.

Series II

Because of the results of the first study, a second series was planned in which the testing conditions were improved. In it there were 12 subjects, 7 from the Laboratory staff and 5 adult visitors to the Laboratory. (Since this study was conducted during the summer months, university students were not available.)

This series was of the same length as the first, consisting of 100 runs of psi-missing and 100 of psi-hitting. The type of test was alternated as in the first study.

The pack of ESP cards in front of the experimenter which were to be guessed by the subject were screened from the latter to control against visual sensory cues. Measures were taken also to prevent the possibility that the subject might get auditory cues from which he could tell which symbol the experimenter recorded and could use

that information to improve his displacement scores. To prevent this, an assistant prepared a number of ESP packs from random number tables before the experiment began. During the test, only the subject's calls were recorded. The record sheet was unscreened; and after each test session, the assistant was given the data to check with her target records. The data were thus checked independently. In other respects, the testing procedure was the same as in the first series.

The results of the second study were similar to those of the first. (See Table 1.) In the 2500 psi-missing trials, there were 467 hits, with a deviation of −33. In the 2500 psi-hitting trials, there were 524 hits, with a deviation of +24. Neither of the deviations is significant, although they are in the expected direction. The CR of the difference is 2.02 ($P = .04$).

From the outset of this series, I planned to check for forward displacement, though this was not done until the series was completed, and the subjects were not informed of this plan. In the 2400 psi-missing trials, there were 466 forward displacements, with a deviation of −14; and in the psi-hitting trials, there were 491 forward displacements, with a deviation of +11. Again, these deviations are in the expected direction, but are not significant.

The backward displacement again could not be analyzed.

Series III

A third study was begun in which a single subject who had performed well in the second study was used. This subject was one of the Laboratory staff members. Again the series consisted of 100 runs of psi-missing and 100 runs of psi-hitting.

The procedure of the third series was identical to that of the second except that the subject made five calls before she was told which of the five were hits. The experimenter recorded each call and at the same time made a small check beside the preceding one to remind herself of which ones were hits.

In this study, the direct-hitting tendency which appeared in the other studies was again repeated. (See Table 1.) In 2500 psi-missing trials, the subject made 472 hits, with a deviation of −28; and in the psi-hitting trials, she had 554 hits with a deviation of +54

(CR = 2.70; P = .0069). The CR of the difference was 2.90 (P = .0037).

The forward displacement effect, however, was different from what it had been in the previous studies. The subject had positive deviations in both types of test. In the 2400 trials for psi-missing, there were 502 hits, with a deviation of +22. In the psi-hitting trials, she had 507 hits with a deviation of +27. Neither of these deviations is significant.

A tentative analysis of the backward displacement can be made in this study, although the experimenter looked at the target card after the call was made. The backward displacement effect can be given only as much credence as would be given to a GESP test in which the experimenter was not screened from the subject.

In the 2400 trials for backward displacement, the subject had 515 hits in the psi-missing trials, with a deviation of +35; and in the psi-hitting trials, 542 hits with a deviation of +62 (CR = 3.16; P = .0016).

Since the subject was told after each five calls which of the calls were successful, she could often have utilized the information regarding the last trial of the five to produce a backward displacement effect on the next call. However, when a correction was applied by removing trials 6, 11, 16, and 21 from consideration, her results were improved. In the remaining 2000 psi-missing trials, she had 438 hits with a deviation of +38 (CR = 2.12; P = .04); and in the psi-hitting trials she had 459 hits, with a deviation of +59 (CR = 3.30; P = .00097). The results indicate there was no utilization of the information given on the five backward displacement trials on which it could have been applied.

Discussion

The results for direct hitting in the ESP tests involved in these studies have been on a fairly suggestive high level. Whether or not the game testing technique is responsible for the results could be determined only by comparative studies with standard tests.

The game, as originally designed, was meant only to test for direct hitting. The displacement effects found in these studies, however, would make it worth while to introduce further modifications so that backward as well as forward displacement could be analyzed.

The modification here introduced, according to which the subject was told of his success after five calls, is a step toward control for backward displacement. The removal of trials 6, 11, 16, and 21 would correct for the information given. Discarding the BT procedure for a modified DT procedure—the experimenter would not look at the cards until all five calls were made—would put backward displacement under clairvoyance rather than GESP conditions. These modifications, plus the introduction of a second experimenter to make an independent record of the subject's calls, would be enough, I believe, to permit the use of the game in formal experiments at the confirmatory level of research.

The main questions raised in the experiment are answered by the evaluations of the total results in each scoring category as shown in Table 1 (together with the backward displacement results of Series III described in the text). It is interesting, however, to ask how consistently the subjects were able to score in the right direction in the two types of game, high-aim and low-aim. To answer this question, the total results of each subject participating in the three series combined were tabulated to show how the directions of deviation of the scores from mean chance expectation in the two games were related. These results are shown in the following table:

	Subjects Aiming High. Successful: Pos. Dev.	Subjects Aiming High. Unsuccessful: Neg. Dev.	Total
Subjects Aiming Low. Successful: Neg. Dev.	8	4	12
Subjects Aiming Low. Unsuccessful: Pos. Dev.	5	2	7
Total	13	6	19

While these figures are small and the distribution does not reach a level of statistical significance, the data do suggest that the individual subjects were successful in directing their ESP in either the

positive or negative direction at will. The results of two subjects are not shown in the above table because their scores on one of the games was precisely at chance expectation. One was positive for the high-aim trials, and the other was negative for the low-aim trials. When these scores are added to those shown in the table, the results indicate that 14 out of 20 of the subjects (70%) were successful in making a positive score on all of their high-aim games combined, while 13 out of 20 (65%) were successful in making a negative score on all of their low-aim games combined.

The demonstration that successful subjects in ESP tests can direct their abilities voluntarily so as to hit or to miss the targets is not new. Such *dirigibility* of ESP was considered as established more than 20 years ago (**6**). Since then, there have been a large number of studies in which certain classes of subjects or certain psychological conditions were found to be associated with negative scoring when the subjects were trying to score positively, but these psi-missing results (**5**) are not to be confused with tests in which the subjects deliberately try to miss the targets. The present investigation may be the first one in which a number of unselected subjects were tested consistently over a series of trials for both high- and low-aim scores. The results suggest that the significant differences found in the total scores were due largely to the success of the individual subjects in scoring in *both* directions, rather than to a situation in which some subjects succeeded only at high-aim while others succeeded only at low-aim.

Although the motivational aspects of an investigation are rather intangible, perhaps it might be worth while to mention some of the features of this test procedure which, in this investigation at any rate, seemed to increase the feeling of confidence regarding the experiment on my own part as well as on the part of long-range subjects. The consistency with which the subjects were able to produce high or low scores at will within the same session was, for the subjects as well as for me, encouraging evidence of the probable operation of ESP in the tests. While, statistically, a given *difference* between the two sets of runs was exactly as significant as the same *deviation* would be when all the runs were for, say, high-aim, the alternation of successful high and low scores seemed especially convincing. I believe this feature helped to keep the testing atmosphere

encouraging rather than frustrating or boring. I cannot say, of course, that others would react as I did to an experiment involving high-aim and low-aim runs, but I think it possible that they would. I hope, therefore, that experimenters who experience any difficulty about maintaining the interest of their subjects in quantitative ESP tests will try making use of this device. Those who do will, I also hope, look for opportunities to compare results in a game situation with those obtained in the ordinary experimental framework.

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ESP AND SIZE OF TARGET SYMBOLS

By Rémy Chauvin

ABSTRACT: Previous experiments to test the possible effect of differences in size of target on ESP scoring were carried out some years ago, and no apparent effect was found. The experiment reported here had the same general objective except that in this case the smaller targets were microfilmed and were tinier than any used previously. The normal size symbols tended to be associated with positive scoring and the small symbols with negative scoring. This difference was concentrated in the first of the two runs, in which it amounted to statistical significance. In the total data, the difference was marginally significant. Since the small targets had the larger (though negative) deviations, the author finds it likely that the differences are due to psychological rather than physical causes. Prof Chauvin is the Director of the Laboratory of Experimental Ethology in Paris.—Ed.

Introduction

In a previous experiment (1) I used a simple method to investigate clairvoyance. The targets were the digits 1 or 2 which were written 12 mm. in height upon cards which measured 2 cm. by 2 cm. Thus, the test was one which provided a fifty-fifty probability basis for success or failure on each trial from the point of view of "chance." The experiment already reported was generally intended to find out whether the procedure adopted was suitable for testing the ESP abilities of children and specifically to study whether the position of the subject in relation to the target seemed to influence the results.

In the present exploratory experiment, my aim was to find out if the size of the target symbols appeared to influence ESP success. As a result of experiments carried out a number of years ago, L. E. Rhine (3) and Pratt and Woodruff (2) found that size per se was not a factor. But they were concerned with sizes which were within the range that could be read without the aid of a magnifying lens. It seemed interesting to carry the investigation of size of target through at least one further step in which even smaller symbols were used.

Subjects and Experimental Procedure

The subjects were myself (age 46) and four children who were already thoroughly accustomed to the general testing procedure.

The youngsters were my son, Jean Pierre (age 13), my nephew Laurence de C. (age 10), and my nieces Myriam (age 14) and Antoinette (age 14). I briefly characterized their temperaments and attitudes toward the tests in my earlier paper. In the present study, as before, I gave small money prizes at the end of the runs, and I was careful to maintain an informal and competitive atmosphere. Each subject worked at irregular intervals, sometimes making several pairs of runs in the same day if he showed no reluctance to do so.

For each test or run, I used a set of 36 of the 2 × 2 mm. cards, each card enclosed in an individual dark opaque envelope. Half of the cards in the set were the same as had been used in the previous experiment: 9 of them had the digit 1 on one side, and 9 had the digit 2. As previously mentioned, these digits were 12 mm. high. On each of the remaining 18 cards there was glued a microfilmed digit 1 or digit 2 (9 of each). The size was so small that it was impossible to read the digit without a lens, and I used a glass which magnified 12 times to check the results in the experiment.

In preparation for each test, the digits were shuffled and placed in the envelopes out of the subject's sight, and the envelopes were thoroughly shuffled and put on a large tray in a rectangular arrangement consisting of six rows and six columns. Each envelope was separated from those around it by a distance of 5-6 cm. The tray was prepared out of sight of all the subjects. They never saw the arrangement of the cards until the checking at the end of each run. Only in the case of myself was there any theoretical possibility of obtaining cues regarding the targets, and it will be seen that my results were the least significant obtained in the experiment.

The subjects were told the purpose of the experiment and knew about the differences in the size of the targets. They were seated with their backs toward the tray in order to exclude all possible visual stimuli from the envelopes. In my previous experiment it seemed to me that this particular position gave better results. As was done previously, the subjects were instructed to call the symbols in ordinary reading order, starting with the row close to their back and going from left to right.

As the experimenter, I did the recording of the subject's calls. Then, at the end of the run, I opened the envelopes and, with the subject looking on, examined the cards and recorded them alongside the calls, taking great care to record the symbol in each instance as "normal" or "microfilmed." The cards were then mixed up, again out of the subject's sight, and replaced face-down in their envelopes and a second run was done immediately following the checking of the first one, since I was interested in a possible influence of the time factor in the test as shown by a comparison of the first runs (Series A) and the second runs (Series B).

Results and Discussion

Each subject did a total of 1296 trials, equally divided between Series A and B and between normal and microfilmed symbols. Thus, for each subject the data divide into four equal groups of 324 trials each: Series A Normal; Series A Microfilmed; Series B Normal; and Series B Microfilmed. For the five subjects combined, each of these groups thus represents a total of 1620 trials. The detailed results are presented in Table 1 in terms of the deviation of the number of hits from the expected chance number (one-half of the number of trials in each instance).

Since the objective was to compare the difference between the two sizes of symbols, the results have been evaluated in Table 1 to show this comparison. It may be seen that the normal size symbols in Series A had a positive deviation (+ 35) and the small symbols were negative (− 69). The difference was statistically significant. In Series B, both size symbols were negative. The CR of the difference here was insignificant, and there was only a marginally significant one when the two series were combined. Taking these results at face value, one may say that there was a difference in the number of hits made on the two kinds of symbols but that this difference was concentrated in the first runs of the session. The higher number of hits was obtained on the normal symbols, but this does not mean that they were physically more effective as clairvoyance targets. Actually, in terms of the size of the deviations from the expected chance number, the negative ones found for the microfilmed symbols are larger than the positive ones

Table 1

GENERAL RESULTS COMPARING NORMAL AND MICROFILMED SYMBOLS

Series A

Subject	Normal Size Symbols				Microfilmed Symbols				CR_d Normal and Micro.	P
	No. Trials	Hits	Dev.	% Success	No. Trials	Hits	Dev.	% Success		
R. C.	324	169	+ 7	52.16	324	168	+ 6	51.85		
Myriam	324	170	+ 8	52.47	324	154	− 8	47.53		
Lawrence	324	159	− 3	49.07	324	138	−24	42.59		
J. Pierre	324	175	+13	54.01	324	131	−31	40.43	3.46	.00054
Antoinette	324	172	+10	53.09	324	150	−12	46.30		
Total	1620	845	+35	52.16	1620	741	−69	45.74	3.65	.00026

Series B

R. C.	324	172	+10	53.09	324	159	− 3	49.07		
Myriam	324	152	−10	46.91	324	161	− 1	49.69		
Lawrence	324	153	− 9	47.22	324	174	+12	53.70		
J. Pierre	324	163	+ 1	50.31	324	154	− 8	47.53		
Antoinette	324	157	− 5	48.46	324	160	− 2	49.38		
Total	1620	797	−13	49.20	1620	808	− 2	49.88		

Total Series A and B

R. C.	648	341	+17	52.62	648	327	+ 3	50.46		
Myriam	648	322	− 2	49.69	648	315	− 9	48.61		
Lawrence	648	312	−12	48.15	648	312	−12	48.15		
J. Pierre	648	338	+14	52.16	648	285	−39	43.98	2.94	.0033
Antoinette	648	329	+ 5	50.77	648	310	−14	47.84		
Total	3240	1642	+22	50.68	3240	1549	−71	47.81	2.31	.021

for the normal symbols. One would therefore surmise that the amount of ESP effect shown was probably slightly greater for the smaller symbols than it was for the larger symbols.

This would indicate that the significance found in the total results of Series A must be explained in *psychological* terms rather than in terms of the difference in physical size as such. My own results as subject contributed almost nothing to the significance of the difference between the two sizes in Series A, while every one of the child subjects gave a higher number of hits on the normal symbols than on the microfilmed ones.

The difference between the two sizes of symbols may be another example of the tendency often observed in psi tests involving two different conditions. One condition yields negative deviations while the other gives positive scores. The consistency with which the children scored negatively on the microfilmed symbols suggests that they considered them harder to perceive by ESP, a psychological attitude that might have been engendered not only by the smaller size but also by the greater time required for checking the results. Since the smaller symbols were not any less effective as ESP symbols, the reaction against them showed up as a *negative* deviation. The decline of scoring to the chance level in the second run of the session (Series B) may only show the importance of strong motivation for the operation of ESP in the test situation. The first run (Series A) was always done while the subject was enthusiastic about working. Due to the time required for checking the results of the first run and arranging the cards for the second test of the session, the atmosphere often seemed to have become less favorable for the second run, and the results appear to bear out this impression. These suggestions are not offered as conclusions, however, since the purpose of the experiment was only to explore for leads for further research.

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A CONTRIBUTION TO EXPERIMENTAL TELEPATHY

By G. H. ESTABROOKS*

Introduction by the Editors of the Journal of Parapsychology:

In this issue the *Journal* is reprinting, with the author's permission, the account of an ESP experiment carried out in the Harvard Psychological Laboratories during the mid-twenties. This work of Dr. G. H. Estabrooks, published as Bulletin No. 5 of the Boston Society for Psychical Research (1927), has long been out of print. The report, consequently, is known to most of today's workers in the field only as a bibliographical item of some historical interest.

There are several reasons why the research deserves to be rescued from comparative oblivion. First, it was done in one of the best experimental psychology laboratories of its time. While the separate aspects of the procedure were not particularly new, the research combined the earlier experimental advances in an effective way. It yielded significant results, and, everything considered, represented a distinctive step in the advance of parapsychology toward its present status as a branch of academic research.

Perhaps the most important feature of the Estabrooks experiment for the present-day investigator is the emphasis the experimenter laid on the importance of the subject's attitude toward the ESP test. From the point of view of history, however, there is little doubt that its main significance is the light it throws on the scientific spirit of the times. Although Estabrooks conducted his experiment forthrightly at the graduate-school stage of his career, the psychological journals would not accept his article and he was forced to fall back upon an obscure publication. He was eventually driven by scientific opinion to an unmistakable reserve concerning this work, as may be seen by noting what he later said about parapsychology in his books: *Hypnotism* (1943), *Spiritism* (1947), and (with N. E. Gross) *The Future of the Human Mind* (1961). It should be noted that a similarly adverse effect of the scientific climate was apparent in the case of other investigators who were pioneers in bringing parapsychological research into the university

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laboratory. For example, Coover presumably hid the significant findings of his ESP experiments at Stanford University under a cloud of statistical confusion. Similarly, the late H. J. F. W. Brugmans became inactive and silent concerning his ESP research after publishing, in the early twenties, the work which he had done under Prof. Heymans at the University of Groningen. These and other instances indicate something of the toll that professional and cultural pressures have exacted from parapsychology.

All the more reason, then, why the actual merits of these early researches should be calmly and carefully reappraised, especially in the light of more recent findings. In the case of the Estabrooks work, the value was greater than the author himself realized, so far as the exclusion of alternative hypotheses to that of ESP is concerned.

In *The Reach of the Mind* (1947), J. B. Rhine for the first time presented the results of a statistical evaluation of the decline in scoring which Estabrooks' subjects showed between the first and second halves of the run. Estabrooks himself had noticed the dropping-off in the scores as the subjects went through the series of 20 trials, but he presented this effect as only an incidental observation. This fact is not surprising, since up to that time very little study had been made of the decline effects in ESP data. Rhine showed, however, that the run decline in the Estabrooks data was clearly significant by ordinary statistical standards, and he pointed out that this finding provided compelling evidence against the counterhypothesis of sensory cues. If any of the subjects had been depending on sensory cues, their scores would not be expected to have declined. This finding in the Estabrooks data is all the more convincing because of the consistency with which similar position effects have been found in other ESP and PK records. Two tables have been added at the end of the reprinted paper to show the actual figures and evaluation of this decline effect (see Editors' Appendix).

FOREWORD

The writer begs to express his deep appreciation to the authorities of the Harvard Psychological Laboratory for the freedom which he has been given in his research.

He would also beg to thank Professor William McDougall for

his constant interest and stimulating criticism, likewise to Dr. W. F. Prince and Dr. G. Murphy for many helpful suggestions.

I. Introduction

Telepathy, if it exists, has not been proven to the satisfaction of all. Indeed we may safely say that if a canvass were made in America, the number of scientific men ready to affirm their belief in telepathy and to back up this affirmation with scientific evidence would be small indeed; certainly a definite minority.

The reason for this attitude is obvious. First, the suspicion of fraud; also a realization of that remarkable sensory and mental acuity which, in some people, may lead to results apparently genuine but in reality wholly due to normal agencies.

Secondly, the extreme difficulty of using true laboratory methods. Up to the present we have worked on the assumption that telepathic power was the gift of a very few. We had first to find a genuine "mind reader" and then obtain his or her coöperation.

Moreover, having found our telepathist and having secured coöperation we frequently found that laboratory conditions were such as to rapidly discourage the medium and that results, even if positive, were not always obtainable on demand.

The efforts of the writer have steadily crystallized along one line, that of obtaining a technique which can be used in any laboratory and which requires as subjects only people who are "normal" in every sense of the word.

Telepathy may or may not exist; but if it does exist we have a right at least to hope that it can be demonstrated under laboratory conditions. Indeed, until this is possible it will be very hard to persuade the orthodox psychologist that it is more than a name for a certain group of frauds and weird coincidences in which he need take no particular interest.[1]

The experiments herein reported are simply a portion of the work done in connection with this problem by the writer during the past two years. As the task became more clearly outlined the work was

[1] The writer is familiar with the excellent work done by Coover and by Troland. While both these experimenters worked with mechanical control, and with material which could be—and was—checked against chance, conditions of experimentation were essentially different. It is hoped that Coover can be persuaded to check these results with the technique herein given.

concentrated along certain lines. Nevertheless practically all the first year and a large portion of the second year were thrown away searching for a technique and following up false cues.

The evidence herein presented is given for what it is worth. The technique described is simple and results obtained by it lend themselves to statistical treatment. The writer regards his work as very strong evidence for the existence of telepathy. Whether others, using the same technique can duplicate these results remains to be seen.

The procedure outlined in this article is simple but it was a very considerable time before it was realized that such a technique could be expected to yield positive results.

Most of the work on telepathy up to the present has stressed certain factors which definitely lead one away from any simplified procedure such as is herein used.

First, only certain people seemed to have this ability to any reasonable extent.

Secondly, in a majority of cases they seemed to require some abnormal state of consciousness—or unconsciousness—in order to demonstrate their gift.

Thirdly, in those cases of telepathy which we term spontaneous there seemed to be a very definite emotional factor entering—the case of the drowning man appearing to his wife is typical.

Finally a very serious criticism could be levelled at most experimental work in that the results did not lend themselves to statistical treatment. The critic could always claim that what was not due to fraud or to some type of hyperæsthesia could be explained on the basis of coincidence.

A series of experiments carried on during the college year 1924-25 under various conditions of sending and receiving and with agent and percipient in various states of consciousness left the writer with the firm conviction that chance could not be invoked as an adequate explanation of all the results obtained.

The problem had crystallized to a certain degree. Thus it became quite evident that neither hypnosis, nor automatic writing in the normal subject—if these can be obtained in a *normal* subject—were yielding results which justified their continued use.

Moreover, the material used had been restricted to playing cards for reasons which will be given later.

The work for the year 1925-1926 was begun with subjects in the normal condition and with playing cards as material. It was intended to interest as many people as possible in the problem and it was hoped that by using various methods a technique yielding positive results might be obtained—if it were possible to obtain such a technique.

In this respect the writer was extremely fortunate and almost by accident hit upon the procedure which he has since been using. It was the habit to follow up every lead which seemed to give the slightest promise and to run every cue to earth as quickly as possible.

One of the college men had undertaken to do some statistical work in the nature of working out chance distributions. For this purpose he assembled three friends in his room and ran through a pack of cards several times. His procedure was to have each of his friends write down a card on a given signal after which he turned a card up from the pack and recorded it. His friends were under the impression that the whole thing was merely for checking purposes, as indeed it was.

The results of this experiment showed that when the cards guessed by each of these three men were compared results were above chance. This lead was immediately followed up and the writer as a result adopted his present procedure of "rapid fire guessing"—thanks to a half hour's original work done by a man who has only the most superficial interest in psychic research.

II. Technique

The technique which the writer now uses is as follows. One or more subjects are obtained who are willing to give half an hour to an experiment in telepathy. They are told that the experiment will not last more than half an hour and that from the nature of the experiment the same subjects cannot be used more than once. Under these circumstances the average college man can be induced to give this much time.

The subject or subjects are then taken to Room 17 in Emerson Hall. Room 17, as it happens, is really a double room. An inner room is separated from the main room by double doors and is rela-

tively sound proof. Noises such as the scraping of a chair, or loud talking can, however, be distinctly heard between the two rooms which make up Room 17. The writer does not wish in any way to stress the point of these rooms being insulated from one another as regards sound.

The subjects of the experiment are told to ask no questions until after it is over, then the writer talks freely with them and discusses any phase of the work on which they choose to question him—within certain limits.

The men are told that in the experiment which is about to take place they are literally to *guess* cards. The operator and one of the men will remain in the outer room and will cut the cards at random with a knife. Cards will be chosen in this way, the deck being reshuffled and the card returned after each choice.

The signalling device is next explained. This consists of a clockwork apparatus in the outer room connected with a telegraph key in the inner room. It is so arranged that the telegraph key is depressed and released every twenty seconds. That is to say, every twenty seconds the key in the inner room gives a double click—the circuit is closed and immediately opened again.

The percipient is now told that on every double click he is to guess a card. He is to write down the *first* card that flashes into his mind, and is to keep his mind off the experiment as far as possible. In the last series he was actually given a magazine article to read during the experiment and told only to think of what was taking place when the key sounded and then only long enough to jot down the *first* card that occurred to him. Throughout the entire experiment everything possible is done to keep the mind of the percipient off the work in hand and to give him the impression that the experiment has no particular value and need cause no worry whatsoever on his part. Needless to say complete abstraction from the experiment is never obtained.

The percipient is then given a slip of paper on which he is requested to write the numbers 1 to 21. He is told that a series of dots and dashes will be sent as a warning that the experiment is about to begin, that twenty seconds after this series the key will give its first double click and will do so twenty-one times, on each of which he is

to jot down a card, giving the entire title of the card, although care is taken not to mention any card in his hearing.

Both doors are then closed and the operator goes into the outer room accompanied by the other subject or subjects. They seat themselves beside the sending apparatus, the other subject is instructed that he is to act as the battery in that he can concentrate on the cards which are to be turned up as intensely as he pleases. No mention is made about his keeping his mind on his friend.

The operator then shuffles the deck of cards, keeping the cards face down all the time. He next sends a short series of dots and dashes, after which he starts the clockwork sending device.

As previously mentioned this apparatus causes the sounder in the next room to click every twenty seconds.

The deck of cards is now cut with a pen knife and the top card chosen. This is turned face up on a white sheet of paper lying on the table in front of the senders, the operator counting himself as one of these. The pack is then shuffled, and ten seconds—about—after the instrument clicks the pack again cut and the next card substituted for the first one, which is returned to the deck. This procedure is carried on until twenty-one cards have been sent. It is then repeated with any other subjects present.

In addition to this a red light was flashed for every red card and turned off for the black cards during the last two series. This light was about six feet away from the two senders. It was hoped that this would reinforce the idea of redness and blackness for the respective cards but the operator thinks it had no effect whatsoever.

In the great majority of cases a subject was used only once for these experiments. This was owing to the simplicity of the material. In some cases a man was used two or even three times—never more. These cases when they occurred were treated as ordinary experiments.

It was found that using playing cards, a person very rapidly acquired certain habits which were fatal to the experiment. Thus he wanted to give the suites in a certain order, would criticise his choices, could not get away from the idea of a certain card, etc. These habits developed very rapidly. In the first series reported on in the following chapter, two or three subjects were allowed to repeat the experiment once or even twice against the better judgment of the operator

and their results included. He soon learned that this procedure was fatal to his technique and discontinued it or warned the subjects beforehand that, good or bad, their results must be rejected.

Another point is the very interesting fact that with practice a certain type of man can be weeded out beforehand. The very worst type of man for an experiment of this kind is in the instructor in psychology; second only to him is the graduate student in the same subject. They simply cannot attack the problem in the proper spirit but insist on criticising the experiment and, much worse, reacting to it as they think it should be reacted to, and not as they are told to react.

One or two experiences put the writer on his guard. Henceforth all instructors who came in to take the experiment out of curiosity were warned beforehand that their results would be rejected. In future experiments the operator will also do considerable rejecting among his ordinary subjects. The moment a person becomes inquisitive, begins asking about types of imagery, criticises the experiment or appears over anxious or excited, the writer feels justified in telling him that his results will not be included. As long as this is done *before* the experiment and the decision adhered to there can be no objection to this procedure.

The very best type of subject is the one who regards both you and the experiment as a nuisance only to be endured for the sake of gratifying curiosity or for the sake of certain other inducements. Thus the writer was careful after each experiment to demonstrate a few sleight-of-hand tricks with the cards and is quite convinced that fully 50% of his subjects came up in order to see the "three card" trick and merely tolerated the experiments as a means to an end. For the purposes of the work in hand this attitude was excellent.

III. Results

Let us now consider the actual results obtained by using this technique, or techniques so closely allied to it that we may lump our results together. Table No. 1 gives a gross analysis of the results presented in the simplest possible form.

This analysis is based on color. Thus we have two colors to deal with and a fifty-fifty chance. In other words pure chance would allow us to guess the cards correctly as to color in 50% of the cases. Table No. 1 simply summarizes results on a color basis, in other

words, how often did the percipient guess a card of the same color as the card on which the agent or agents were concentrating

Series No. 1 contains twenty-eight experiments of twenty or more cards each. In other words the experiment described in the preceding chapter was performed twenty-eight times, in the great majority of cases on separate individuals.

There are one or two points about this series which deserve special consideration. First, the clock apparatus was not used here. A telegraph key replaced this and was controlled by the operator. However, it was used under conditions which would seem to make any code of signalling quite impossible. The card was exposed as previously described and an elastic band slipped over the key thus closing the circuit and giving a *single* beat. When the next card was exposed this band was removed, the sounder was thus released and this beat, again *single* was the signal for the next card. Thus for each card there was a single beat given as a signal for a guess.

This introduces a time factor. Timing here was not accurate nor was there any reason why it should be. The cards were exposed roughly at the rate of three per minute and since new subjects were being used for the great majority of the experiments it hardly seems possible that they could, in a single experiment, have got onto a subconscious time code. It is probably quite impossible to vary the *intensity* of this beat by any manipulation of the key, so this may be ruled out.

The cards were sent when the operators felt he had them clearly visualized and felt that peculiar "inner urge" of which more later.

The operator acted as agent in the majority of the cases but frequently there were one or even two other agents with him who in turn became percipients.

Another point of interest in this series is that a rest was called after the first ten cards and the percipient engaged in conversation on any subject of interest *not* bearing on the experiment. This rest should be borne in mind in view of certain results which come out later.

Series No. 2 contains thirty-three cases of twenty cards each. Here each experiment was conducted with a different percipient and the conditions were essentially those of the preceding chapter with the single exception that the percipient was given nothing to read

TABLE No. 1. COLOR

Number of experiments in various series, number of correct guesses chance would allow, and number of correct guesses actually obtained as to the color of the various cards. Ordinary playing cards used.

	SERIES 1 (28 Cases)		SERIES 2 (33 Cases)		SERIES 3 (22 Cases)		TOTAL (83 Cases)	
	Chance	Actual	Chance	Actual	Chance	Actual	Chance	Actual
First 10 cards..	140	169	165	196	110	131	415	496
Second 10 cards	140	159	165	168	110	115	415	442
Total (20 cards)	280	328	330	364	220	246	830	938

TABLE No. 2. SUITE

Same data as in Table No. 1, but analyzed as to suite.

	SERIES 1 (28 Cases)		SERIES 2 (33 Cases)		SERIES 3 (22 Cases)		TOTAL (83 Cases)	
	Chance	Actual	Chance	Actual	Chance	Actual	Chance	Actual
First 10 cards..	70	88	82.5	101	55	67	207.5	256
Second 10 cards	70	83	82.5	73	55	61	207.5	217
Total (20 cards)	140	171	165	174	110	128	415	473

during the experiment. The clockwork timing device was employed and there was no rest after the first ten cards.

Series No. 3 contains twenty-two cases of twenty cards each. Here conditions were exactly those described in the preceding chapter.

Under total we have all our cases grouped together.

Series No. 1 occurred from Oct. 21, 1925, to Nov. 23, 1925.

Series No. 2 occurred from Mar. 13, 1926, to Mar. 31, 1926.

Series No. 3 occurred from Apr. 12, 1926, to May 18, 1926.

The experiments occurred in the morning, afternoon, or evening, largely in the afternoon. It must be remembered that the writer was merely interested in *demonstrating* telepathy and when he found that his results seemed consistent irrespective of the time of day, he took his subjects when he could get them.

The subjects for Series No. 1 were largely personal friends of the

writer, whereas those for Series Nos. 2 and 3 were largely students —mostly undergraduates—obtained through the courtesy of Professor McDougall who allowed the operator to canvass two of his classes for material.

In Series No. 1, Table No. 1, we had twenty-eight cases. Considering the first ten cards we would have 280 choices altogether. Pure chance would allow us 140 coincidences as to color. We actually obtain 169. Similarly in the second ten cards of this series we obtain 159 coincidences as to color against 140 allowed by chance, and in the first twenty cards we get 328 against 280. The other columns are to be similarly interpreted.

Table No. 2 gives us the same analysis with regard to suite.

We here see several points clearly which are perhaps even better brought out when we express our results in terms of probability.

We are probably justified in using the formula for the Standard Deviation of the Binomial Expansion to obtain the Standard Deviations of normal groups of the size herein used. This formula is $\sqrt{(kpq)}$. (See Brown and Thompson, Essentials of Mental Measurement, page 30.) Here "p" is the probability of an event succeeding, "q" of its not succeeding and "k" the number of trials.

Roughly we may say that the distribution represented by the Binomial Expansion approximates that of the Gausian Curve and may say that the Probable Error is .6745 times the Standard Deviation. Expressing our deviations from the normal in terms of the Probable Error we can arrive at a simple expression in terms of chance.

Thus (Rugg; "Statistical Methods Applied to Education," p. 231).

"The chances that the true value . . . lies within

± P. E. are 1 to 1.
± 2 P. E. are 4.5 to 1.
± 3 P. E. are 21 to 1.
± 4 P. E. are 142 to 1.
± 5 P. E. are 131 to 1.*
± 6 P. E. are 19,200 to 1."

If we are allowed to treat our results on this basis we have the following, using Table No. 1 for our data.

* These figures should be 1329 to 1.—Ed.

Series No. 1:

Excess of first 10 cards over chance is 5.14 times the P. E.
Excess of second 10 cards over chance is 3.38 times the P. E.
Excess of total (20) cards over chance is 6.04 times the P. E.

Series No. 2:

Excess of first 10 cards over chance is 5.05 times the P. E.
Excess of second 10 cards over chance is .49 times the P. E.
Excess of total (20) cards over chance is 3.95 times the P.E.

Series No. 3:

Excess of first 10 cards over chance is 4.02 times the P. E.
Excess of second 10 cards over chance is 1.01 times the P. E.
Excess of total (20) cards over chance is 3.68 times the P. E.

Total:

Excess of first 10 cards over chance is 8.35 times the P. E.
Excess of second 10 cards over chance is 2.78 times the P. E.
Excess of total (20) cards over chance is 7.90 times the P. E.

We thus see that our results are well ahead of chance; in fact, no one would claim that these figures can be explained merely by coincidence. They are far too consistent and the chances are far too great.

One very interesting point well worthy of careful note is the fact that our first ten cards always give more significant results than our second ten cards. This point could hardly be more clear cut and, in the opinion of the writer points to the fact that the faculty underlying this phenomena, whatever it may be, is very easily fatigued—at least with this technique.

Only in one of the series do the second ten cards make a significant showing and this is in Series No. 1. It will be recalled that in this series a rest was allowed after the first ten cards which was not permitted in the other two series.

The number of coincidences in the first ten cards as compared with the second ten cards, taken merely on the basis of superiority, equality or inferiority illustrates our points graphically.

Coincidences in first ten cards as compared with coincidences in second ten cards:

	Color		
	Ahead of	*Equal to*	*Less than*
Series No. 1.....	16	3	9
Series No. 2.....	19	4	10
Series No. 3.....	11	7	4
Total.....	46	14	23

	Suite		
	Ahead of	*Equal to*	*Less than*
Series No. 1	15	3	10
Series No. 2	19	5	9
Series No. 3	11	4	7
Total	45	12	26

Series No. 1 taken as a whole or the first ten cards of Series Nos. 2 and 3 yield results which cannot be due to chance.

We must bear in mind certain facts with reference to Series No. 1. The subjects were friends of the operator; this gives an emotional element which we cannot afford to overlook. The subjects were also given to understand that they were being merely used as checks in a mathematical sense. This white lie tended to remove any tension. In the later series it was found necessary to be frank about the nature of the experiment in order to maintain interest and obtain subjects.

Again the operator had control of the sending apparatus. Within certain limits he literally gave the signal when the spirit moved him, that is to say when he had a "hunch" that he had the card properly visualized and had a certain subjective feeling of certainty that it was going through.

The writer places considerable stress on this point. Experiments of the preceding year seemed to show that this particular state of consciousness was a great assistance.

All through Series Nos. 2 and 3 the writer could not help feeling a certain personal resentment against his mechanical sending device. It *was* mechanical and would not wait for proper visualization or for that subjective feeling of certainty. The operator did his best to accommodate himself to the machine but was often painfully aware that the signal was being given at times when another ten seconds would have meant much better conditions, at least, from his point of view.

If we are permitted to group our results together as we have done in the last column of Table No. 1 it is clearly evident that we are even more decidedly ahead of chance. This point is not insisted on, but in the opinion of the writer we are entitled to do so.

The writer has not treated Table No. 2 on a chance basis. An inspection of this table will show that suite is behaving very similarly to color but owing to our smaller chance of getting suite correctly the

chances as expressed by our method would not be mathematically significant.

Table No. 3 also gives us a graphic representation of our tendencies. Thus, as an example, "ahead of chance in the first four cards" as to color would mean that three or more cards were guessed correctly as to color, equal to chance that two were guessed correctly and below that one or less were guessed correctly. With suite for the same example ahead would be two or more suites, equal to one, below none.

TABLE NO. 3

Relation of actual results to chance in various sections of the different experiments.

	SERIES 1 (28 Cases)		SERIES 2 (33 Cases)		SERIES 3 (22 Cases)		TOTAL (83 Cases)	
	Color	Suite	Color	Suite	Color	Suite	Color	Suite
In First 4 Cards								
Ahead of Chance	20	18	16	16	9	7	45	41
Equal to Chance	5	7	11	12	6	11	22	30
Below Chance	3	3	6	5	7	4	16	12
In First 8 Cards								
Ahead of Chance	16	17	19	16	11	11	46	44
Equal to Chance	6	7	9	11	4	6	19	24
Below Chance	6	4	5	6	7	5	18	15
In First 12 Cards								
Ahead of Chance	18	21	20	17	12	11	50	49
Equal to Chance	6	2	6	10	4	6	16	18
Below Chance	4	5	7	6	6	5	17	16
In First 16 Cards								
Ahead of Chance	20	22	21	18	14	14	55	54
Equal to Chance	4	3	5	7	2	3	11	13
Below Chance	4	3	7	8	6	5	17	16
In First 20 Cards								
Ahead of Chance	20	21	21	14	14	13	55	48
Equal to Chance	5	5	3	8	3	6	11	19
Below Chance	3	2	9	11	5	3	17	16

While we cannot express this as readily in terms of probability Table No. 3 is, in its way, quite as graphic as are the previous tables.

A further very interesting series should be reported at this point. This series was conducted between Rooms 11 and 17 of Emerson Hall. These rooms are about sixty feet distant from each other. The percipient was seated within the inner room of Room No. 17 and

the agent or agents were seated in Room No. 11. The technique was the same as for Series No. 1, that is to say a telegraph key was employed instead of the clock apparatus, and a rest allowed after the tenth card.

We will call this Series No. 4. It was run off between October 21, 1925, and November 23, 1925. The subjects were for the most part those men who had taken part in Series No. 1, this forming, so to speak, a second experiment which was put through at the same sitting. That is to say the subjects were first put through the tests of Series No. 1 and then immediately given the same experiment over again with the agents removed to Room No. 11.

All the doors, four in number, were closed between the rooms and under these circumstances there could be very little auditory communication between them. Results were wholly negative.

Thus thirty-two experiments were put through.

As to Color

	Chance would give	*Actually we get*
First 10 cards	160	154
Second 10 cards	160	153
Total (20) cards	320	307

The Probable Errors are respectively for the

First and Second 10 cards	± 6.0
Total (20 cards)	± 8.5

In this series we actually fall below chance as to color, but not significantly so.

As to Suite

	Chance would give	*Actually we get*
First 10 cards	80	76
Second 10 cards	80	54
Total (20) cards	160	130

So far as suite is concerned we are again below chance, quite definitely so as regards the second ten cards.

Analyzing this series on the same basis as Table No. 3 we get the following:

In First 4 Cards:

	Color	*Suite*
Ahead of Chance	8	8
Equal to Chance	11	14
Below Chance	13	10

In First 8 Cards:

	Color	*Suite*
Ahead of Chance	9	10
Equal to Chance	8	11
Below Chance	15	11

In First 12 Cards:

	Color	*Suite*
Ahead of Chance	10	8
Equal to Chance	7	7
Below Chance	15	17

In First 16 Cards:

	Color	*Suite*
Ahead of Chance	10	8
Equal to Chance	7	7
Below Chance	15	17

In First 20 Cards:

	Color	*Suite*
Ahead of Chance	10	8
Equal to Chance	8	6
Below Chance	14	18

We noticed in previous series that the success tended to be emphasized in the first ten cards. In this series we find a very interesting fact. The entire twenty cards are below chance but it is interesting to note the relations which success in the first ten bears to success in the second ten.

As to color we find successes in the

First ten cards exceeding the second ten in 9 cases.

First ten cards equal to the second ten in 9 cases.

First ten cards below the second ten in 14 cases.

which is the *reverse* of the tendencies previously noted but, as to suite, we find successes in the

First ten cards exceeding the second ten in 21 cases.

First ten cards equal to the second ten in 5 cases.

First ten cards below the second ten in 6 cases.

which is the *same* tendency we have before noted.

The writer is inclined to ascribe the failure between Rooms Nos. 11 and 17 as due largely to the distance factor with perhaps an element of greater insulation entering in or some factor such as adverse auto-suggestion.

On the suggestion of Professor McDougall the writer attempted to retest as many as possible of his former subjects under conditions which would rule out all possibility of telepathy and use these results as a check. Unfortunately the men thought otherwise and would not cooperate. While they were perfectly willing to give a half hour once, they were by no means willing to do so twice much to the chagrin of the operator.

However, at the suggestion of one of the instructors who has been interested in the course of the experiment the writer worked out a statistical check on his results. He wished to find out just what would be the result if the responses of the subjects were checked up against a criterion which was obviously quite beyond any influence of telepathy. Thus if we could check their responses up against random drawings from a pack of cards with which they clearly had no connection we would expect them in the long run to approximate chance.

Actually the writer checked their responses up against the fifth card ahead in the series. Thus the response to stimulus No. 1 was checked against the actual card drawn for stimulus No. 6. As this card was actually turned up five cards *after* the person guessed for stimulus No. 1 it is inconceivable that any real connection could exist between this card and the response in question. Results show a very close approximation to chance, and are given in Table No. 4.

Similarly a checking up of the entire series shows that the various suites and colors are distributed quite normally in as far as the operator was concerned. That is to say the colors show approximately a fifty-fifty distribution and the suites correspond.

Finally the writer suspected that perhaps the previous cards drawn by the agent might have something to do with the responses of the percipient. Thus perhaps a black card after a red card might go through more easily than a black card following a black card.

The total series was broken up as follows:

Black after 1 Red card.	Red after 1 Black card.
Black after 2 Red cards.	Red after 2 Black cards.
Black after 3 Red cards.	Red after 3 Black cards.
Black after 4 Red cards.	Red after 4 Black cards.
Black after 5 Red cards.	Red after 5 Black cards.
Black after 6 Red cards.	Red after 6 Black cards.
Black after 1 Black card.	Red after 1 Red card.
Black after 2 Black cards.	Red after 2 Red cards.
Black after 3 Black cards.	Red after 3 Red cards.
Black after 4 Black cards.	Red after 4 Red cards.
Black after 5 Black cards.	Red after 5 Red cards.
Black after 6 Black cards.	Red after 6 Red cards.

TABLE No. 4

Results of checking guesses against card drawn by operator five guesses later. Results here given as to color.

	No. of coincidences in guessing the various cards of the different series.																				
	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	XIII	XIV	XV	XVI	XVII	XVIII	XIX	XX	
Series 1 (Chance 14)	16	12	13	14	14	16	12	16	11	16	12	14	11	16	10	7	8	15	15	19	267
Series 2 (Chance 16½)	18	13	14	14	18	23	14	16	16	11	17	11	16	12	15	19	13	17	15	19	311
Series 3 (Chance 11)	9	12	11	14	12	15	11	10	13	7	8	9	11	10	9	8	14	8	10	14	215
Series 1, 2, 3 (Chance 41½)	43	37	38	42	44	54	37	42	40	34	37	34	38	38	34	34	35	40	40	52	793
Series 4 (Chance 16)	14	11	13	16	15	14	19	20	20	22	18	19	12	18	20	17	15	17	17	20	337

TABLE No. 4 (Continued)

Results as to Suite

	No. of coincidences in guessing the various cards of the different series.																				
	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	XIII	XIV	XV	XVI	XVII	XVIII	XIX	XX	
Series 1 (Chance 7)	5	6	4	10	11	11	4	12	3	9	6	7	4	7	5	2	2	8	10	10	136
Series 2 (Chance 8¼)	10	5	8	7	7	13	7	8	10	5	9	6	3	8	6	11	7	4	10	6	150
Series 3 (Chance 5½)	5	4	8	7	4	9	4	6	5	6	7	5	6	5	5	4	6	5	5	10	116
Series 1, 2, 3 (Chance 20¾)	20	15	20	24	22	33	15	26	18	20	22	18	13	20	16	17	15	17	25	26	402
Series 4 (Chance 8)	10	5	6	8	6	10	12	11	11	10	8	4	6	7	7	5	5	8	11	5	155

In as far as the writer can see the positive results obtained are in no way correlated with any such order of occurrence.

Lastly we had the question of the influence of the form of the cards which the agent had been gazing at previously. Thus a club following a club might come through easier than a club preceded by a diamond or a spade.

With this point in view the entire series was again broken up into the following categories:

Club following Club.
Club following Diamond.
Club following Heart.
Club following Spade.
Diamond following Diamond.
Diamond following Club.
Diamond following Heart.
Diamond following Spade.
Heart following Heart.
Heart following Club
Heart following Diamond.
Heart following Spade.
Spade following Spade.
Spade following Club.
Spade following Diamond.
Spade following Heart.

Here again, in as far as the writer can see, results are entirely negative.

IV. Discussion

We must bear a number of points clearly in mind when dealing with the subject of telepathy. It seems at least reasonable to suppose that if telepathy does exist it has a normal distribution throughout any section of humanity both as to the receiving and as to the sending end. Thus some of us may be excellent agents, some excellent percipients, but it is highly probable that the majority would be *normal* —whatever that word might signify applied to such a subject. At any rate it is the writer's contention that if such a phenomenon does exist it exists in all humanity and not only in half a dozen highly sensitive mediums.

Consequently we must search for a technique which will take this very weak tendency and magnify it until we can claim proof. Needless to say, this is not easy.

In the laboratory we cannot get the intense emotional state which seems so necessary to a really conclusive demonstration. Neither do we know of any abnormal state of consciousness which we can induce and so guarantee results.

On the other hand I think we must claim positive knowledge on one point, namely that we at least realize our ignorance of the laws which govern its manifestation—and there is some grace even in this state.

Thus it seems to the writer quite unreasonable that we should expect telepathy to act without regard to time or space. From his own results he is convinced that both factors are of great importance. We do not know how long a period of concentration is best on the part of the agent or how long to allow for the percipient to get the impulse. In the opinion of the writer, work in the same room will yield better results and work in widely separated rooms yield practically none—that is, with this technique.

Possibly this is due to the space factor. Possibly due to insulation. It seems a little unreasonable to suppose that "thought waves" can surmount all obstacles with equal readiness.

Again it may be due to auto-suggestion in some form or other. As the writer was merely interested in demonstrating telepathy and did not attempt to vary his results too much until he had succeeded in the former undertaking, he is very vague on these points.

From the results of his own work he feels justified in making the following suggestions for future research:

First, we must work out a technique to *demontrate* telepathy, if it exists. At present we are mainly interested in this phase of the work and should not attempt too seriously to understand all the laws which govern its appearance until we can show conclusively and under laboratory conditions that it does appear.

Secondly, we must deal with material that is strictly subject to statistical treatment. Playing cards are excellent, mainly because everyone is familiar with them. The writer found that if he attempted this same technique with three colored lights—red, green and blue—his results were negative.

The reason in his opinion is probably as follows: With this procedure we require a minimum of conscious interference. Any person can instantly produce the name of a card. On the contrary, simple as the task is, the subject has to keep these three colors in mind. They are at all times more or less in consciousness. When he receives the signal his first impulse is to give a color. He is just as liable to say brown or yellow. For an instant he is held up and has to criticise his own attitude. This, in the opinion of the writer, is fatal. With cards it is different. There are only fifty-two, he can choose any and is perfectly familiar with all.

The third suggestion is that our technique should be one which uses normal human beings in the normal state *if this is possible,* if for no other reason than that they are comparatively easy to obtain. Moreover by using a technique wherein no subject is used more than once we eliminate to a large degree the hypothesis of fraud.

Fourthly, our technique should be mechanically controlled. In the writer's opinion this may present grave drawbacks in that a machine is a very impersonal affair and the personal factor is probably here of immense importance. However, our technique must eliminate all hypothesis of a subconscious code between agent and percipient.

Fifthly, we must in as far as possible eliminate all charges of hyperæsthesia. The technique here advanced does away with visual hyperæsthesia but the fact remains that the rooms are not sound proof; in fact under the most ideal conditions any rooms could only be *relatively* sound proof. An unconscious code of shufflings, scrapings, etc., or subvocal enunciation might be advanced as an explanation. It is rather far fetched and where we use new subjects each time we would be restricted to the subvocal enunciation. In the writer's opinion it is quite impossible as an explanation.

Sixthly, we must put aside any preconceived notions as to how telepathy should work. Using this particular technique it would appear that the faculty which underlies it is extremely susceptible to fatigue, that it works best with a minimum of conscious interference and that any strain or excitement on the part of the percipient is a pretty certain indication of failure.

The writer obtained his best results in Series No. 1 wherein he had a certain amount of personal touch with the signalling apparatus,

yet under conditions which rendered a subconscious code quite impossible. Also in these experiments the subjects were mostly his personal friends and he actually told them that their work had nothing whatsoever to do with telepathy; they were merely "statistical checks." This naturally did away with any worry. Unfortunately in order to obtain subjects in his later series he found it necessary to be strictly truthful on the later point, so that the subjects realized that they were taking part in a serious experiment. But in this first series we have all conditions eminently favorable.

Finally we should bear in mind that there are literally a dozen different ways of approaching the problem any one of which may lead to its solution. Perhaps some state of "drug" consciousness may facilitate communication, some of the conscious states associated with the various forms of hysteria may supply the key, some form of the "ouija" board may give a conclusive answer and it may be found that only as we take into consideration such factors as sex and the various emotional ties resulting therefrom can we get our solution.

The writer merely offers these suggestions and his own experimental work for what they are worth. Time and a great deal of careful investigation alone will tell whether they are worth anything. However it must be insisted upon, that, if this technique is used it must be used in strict accordance with instructions. The timing of the cards is very important otherwise the cards tend to become confused.

If a second card is exposed, say two or three seconds after the signal for the previous card, a certain type of subject who is rather slow on reaction time will not yet have made his choice and will be influenced by this card. On the other hand, if the card is not exposed until two or three seconds before the stimulus this does not give the other card time to recede from the agent's consciousness.

A fundamental point is the ability to get in quick and friendly rapport with the subjects and put them at their ease as to the experiment, a subject who is nervous or hypercritical or covertly hostile is worse than no subject at all.

Finally, the writer places great stress on his own state of mind when sending. Clear visualization and a confidence that the impression is going through are great aids—at least in as far as he can see. This cannot, of course, be obtained in the other senders but they can at least be told to concentrate.

APPENDIX NO. 1

Herein are given the crude scores of the experiments quoted in the previous pages. The number of coincidences is given which was obtained in the total guesses of each individual card in the various series

TABLE No. 5. SUITE

	No. of coincidences in guessing the various cards of the different series.																				
	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	XIII	XIV	XV	XVI	XVII	XVIII	XIX	XX	
Series 1 (Chance 7)	16	8	11	10	7	10	6	6	5	9	13	7	10	10	10	7	6	6	4	10	171
Series 2 (Chance 8¼)	14	13	8	10	6	12	9	11	8	10	7	7	8	7	12	8	7	8	3	6	174
Series 3 (Chance 5½)	12	4	7	3	8	6	4	8	9	6	4	4	6	5	11	5	10	5	7	4	138
Total 1, 2, 3 (Chance 20¾)	42	25	26	23	21	28	19	25	22	25	24	18	24	22	33	20	23	19	14	20	473
Series 4 (Chance 8)	10	9	8	6	6	10	7	9	5	6	2	5	6	6	6	6	9	6	1	7	130

TABLE No. 5 COLOR

	No. of coincidences in guessing the various cards of the different series.																				
	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	XIII	XIV	XV	XVI	XVII	XVIII	XIX	XX	
Series 1 (Chance 14)	24	19	17	21	13	19	14	13	14	15	18	15	15	15	18	15	18	17	10	18	328
Series 2 (Chance 16½)	24	21	14	17	18	21	19	19	21	22	17	16	17	20	21	19	20	17	11	10	364
Series 3 (Chance 11)	17	9	12	10	15	11	12	13	16	16	10	12	13	9	14	9	15	8	12	13	246
Total 1, 2, 3 (Chance 41½)	65	49	43	48	46	51	45	45	51	53	45	43	45	44	53	43	53	42	33	41	938
Series 4 (Chance 16)	18	17	15	10	17	17	13	12	14	21	12	15	15	15	16	16	20	17	12	15	307

Editors' Appendix

TABLE A

Evaluation of the Difference Between the Top and Bottom Halves of the Column as Regards the Scoring on Suit

Pooled Column of All Four Series

	Top Half	Bottom Half	Total
Hits on Suit	332	271	603
Misses on Suit	818	879	1697
Total	1150	1150	2300

$\chi^2 = 8.36$, 1 d.f.; $P = .004$

TABLE B

Evaluation of the Difference Between the Top and Bottom Halves of the Column as Regards the Scoring on Color

Pooled Column of All Four Series

	Top Half	Bottom Half	Total
Hits on Color	650	595	1245
Misses on Color	500	555	1055
Total	1150	1150	2300

$\chi^2 = 5.30$, 1 d.f.; $P = .021$

RESPONSES TO THE FORUM ON PHYSICALITY AND PSI

Discussion of Dr. Pratt's Paper

Dr. Pratt incisively identifies the crucial role in my argument of a broad conception of the domain of physics. Now, I do not take "physics" as *synonymous* with "science," which would clearly be an error. Yet physics is, in an important sense, *co-extensive* with science. This simply means that physics is that subject which undertakes to provide *an* account (not the *only* account) of *any* phenomenon that occurs in the perceptible universe. The appreciation of art is not, as a subject, part of physics; but as a phenomenon, it occurs in this world, and physics would be deficient if it upheld principles contravened by this phenomenon. We could simply say, "Look here, your laws are *not* universal: every time someone responds to a painting, he acts in a way that is incompatible with your laws." And that would be the end of the laws. We might say that physics is not a potential *substitute for* aesthetics (or parapsychology) but is perennially *dependent on it*. This is what I mean when I say "psi phenomena are psychological phenomena and hence—in certain aspects—physical phenomena." Psi occurs in people, in certain physical circumstances; we can give a physical description of the exact observations and experimental conditions from which we infer psi, and physics must be able to reconcile these descriptions since *they* are descriptions of purely physical occurrences. If psi *must* be inferred from them, psi is therefore a *physically* required hypothesis. The principles of physics do not include those of art criticism (or psi), but they must not be incompatible with the behavior of art critics (or successful psi subjects). In this way, physics must be co-extensive with, but is not identical with, other subjects.

The same point could be made by criticizing Dr. Pratt's proposed definition of physics as concerned with "time-space-mass relationships." Energy must be included, and entropy; and hence degrees of order and disorder; hence information-content and correlations between messages, i.e., ESP phenomena.

Dr. Pratt suggests that everyone who defines psi as the science of nonphysical aspects of Nature takes "nonphysical" to refer to the domain beyond *present-day* physics. This will not do at all. We have recently seen revisions in physical theory to handle the essentially indeterministic behavior of electrons, at least as shattering a revolution as would be needed to handle precognition. Does that mean parapsychology should have (until recently) included the phenomena of electron diffraction? Certainly not; nor does this mean we must fall back on a "definition by listing." We can build on Dr. Pratt's *second* definition which he unwittingly uses as if it were equivalent to the first. Parapsychology is the study of those *psychological* phenomena which appeared to contravene the physical theory of the mid-twentieth century. I have added "appeared to" because I am not sure that telepathy would cease to be part of our subject if it turns out to be an observable consequence of some esoteric quantum principle exemplified in no other part of psychology. If parapsychology is really part of psychology, telepathy would still be part of the business of parapsychology. If it is the field of the *truly* nonphysical (as of 1961) part of psychology, then it may shrink away as the consequences of present physical theories are developed. In any case, it should not be defined as the field of the *perennially* nonphysical part of psychology, since, for the reasons given, this practically guarantees its nonexistence, though proof may be delayed. It is worth contemplating the fate of hypnosis in the history of parapsychology, and the current status of faith-healing, which, under the guise of "the placebo effect," is now in the field of ordinary medical research.

Reflection will show corresponding difficulties with Dr. Pratt's definition of mental-*physical* dualism, and his view that free will requires "an order of mental causation which is to some degree independent of the *physical* order" (my italics). The latter view seems to me, on other grounds, quite unconvincing—though many have accepted it. And finally, I see no reason to think that the "nonphysical" (in Dr. Pratt's sense) aspects of personality are any more likely to survive than the physical ones if both have always and only been found associated with living humans.

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Comments on Dr. Scriven's Comments

Dr. Scriven says that psi is physical because it is always associated with people with physical bodies, involves physical responses, conveys information regarding physical events, etc. He is defining psi in terms of its incidental physical accompaniments. By the same logic, would he say that electricity is heat or light or rotary motion? Surely it is a main business of science to distinguish between a process and its phenomenological manifestations.

Psi phenomena are nonphysical not for the lack of any physical explanation—which is obvious enough—but because the type of operation involved in psi performance defies physical explanation. This is most readily seen in precognition, since physics has no evidence or place for it. But physical concepts are also unable to deal with PK and ordinary ESP when the full range of the findings are taken into account.

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Physicality and Psi Phenomena

Coming from modern physics to parapsychology, I, unlike most of my fellow-parapsychologists, do not consider the two fields of experience incompatible. From my point of view, physics embraces the range of occurrences below the biological level, while psi phenomena transcend mere biology. There is still a tendency, stemming from the antiquated materialistic hypothesis, to reduce all phenomena to the qualities of their components. But a simple mathematical consideration shows the impossibility of such a procedure. Mathematically, the qualities of the components correspond to differential equations, the integral of which is the corresponding phenomena. Every mathematician knows that such a process of integration involves additional values, characterizing the limits of the whole, which by no means can be derived from the corresponding differential equations. These characteristics of the whole, i.e., its individual shape, special quality, and position in time and space, is something new, entering our universe with the birth of an entity and leaving it again with its death. In other words, "the whole is more than the sum of its parts."

Besides minor additional traits of a composite individual compared with its building units, there are some major steps in the hierarchical structure of our world, representing the origination of something basically new relative to its components. Beginning with quantum mechanics, an elementary particle, e.g., an electron, consisting of a chain of millions of similar elementary events (h) represents by this repetition the quality of duration, unknown in its constituent world of individual events. The quality of a certain pattern, characterized by a certain arrangement of individual building blocks, enters the universe in the composite particles: the atoms and molecules. These particles are not yet really embodied, their local position being still indefinite and only marked by their emerging probability. The locally fixed body-state of a crystal, a droplet, or a bubble signifies a basically new quality: it enters the universe, if the total of its individual emerging events is overlapping, limiting its range of appearance to a well-defined space. A still higher quality is introduced into the world by the phenomenon of the "open systems" ("dynamids"), such as flame, eddy, river, center of reaction, etc. These entities, the highest ones of the non-biological world, represent, in addition to their embodiment, the qualities of regeneration, of power, of input and output. Compared with these "dynamids," the living cells, plants, and lower animals are characterized by an innate program, co-ordinating the organismic processes, each of which belongs to the dynamid level. The personality level represents something basically new inasmuch as personality is a generator of programs, i.e., time-including ideas, while mere biological individuals cannot but follow the programs implanted in them. A hyperbiological level is characterized not only by the faculty of idea-production, but also by a judgment-based choice between competing ideas. On this level ideas are not the essence but only the raw material for the individual. Experience shows that personality traits, defined this way, are to be found occasionally also in higher animals and are not yet fully developed even in humans. Psi phenomena can be considered a special kind of personality tool.

As to the connection between the different levels, all the innate laws of the lower levels are to be found in the functioning of the higher individuals, but not all the characteristics of the latter are to

be found on the lower levels. To express their characteristic tendencies, the higher individuals make use of the indeterminacy gap in the laws governing the lower levels. That is why all faculties of the higher individuals must pre-exist as a possibility within the range of their components, just as the players of a symphony cannot surpass the phonic range of their instruments. The same is valid for psi faculties. While classic physics denied the existence of corresponding atomic qualities, modern physics, in the form of wave mechanics, knows the "tunnel effect" (dominating, for example, radioactive decay) and the "unemerged state of matter," following and preceding the individual emerging events of photons, elementary particles, atoms, and molecules. In this intermediate state the particles, characterized by their emerging probability, are not locally fixed, but have basically equal access to every spot in the universe. Their change from one spot to another within their probability range of emergence is a kind of phase-shifting in their probability wave pattern, the phase-shifting velocity (u) surpassing the velocity (c) of light insofar as the velocity (v) of the emerged particle is lower than that of light, according to the well-known equation: $uv = c^2$. The local change of emergence cannot be traced: the particle simply disappears at one spot and reappears at another one. If we grant this faculty of low-level individuals to an individual of personality level or to an object of his interest, we have the physical bridge not only to telepathy, clairvoyance, and telekinesis, but also to "apports" and materialization phenomena if they occur. The one working hypothesis we have to introduce here is that personal interest tends to shift the emerging probability of the individuals involved.

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BOOK REVIEWS

CHALLENGE OF PSYCHICAL RESEARCH: A PRIMER OF PARAPSYCHOLOGY by Professor Gardner Murphy (with the collaboration of Laura A. Dale). New York: Harper & Bros., 1961. $6.00.

This book is one of a series called "World Perspectives" edited by Ruth Nanda Anshen. The aim of the series is described in a preliminary note by the general editor as that of presenting "powerful thinkers, unafraid and unimpeded, fiercely and unremittingly dedicated to the universal, unitive yet paradoxical meaning of life which may emerge out of the disciplined vision of reality." The authors in the series include many of high distinction in both science and religion, including Heisenberg, Suzuki, Hoyle, and Tillich.

None of us would doubt the wisdom of the choice of Gardner Murphy as a writer in parapsychology able to acquit himself with credit in such a company. One may feel more doubt about the possibility of anyone being able to fulfil for parapsychology the purpose of the series. Who in the present embryonic stage of the subject can say how it is going to contribute to the total picture of the world? One could not tell that unless one knew beforehand what research in parapsychology is going to find out. Is it going to make the meaning of life more unitive or more paradoxical? The answer to this must be found by research—not, at this stage, by thinking, however unimpeded and unafraid this thinking may be.

It is, I think, a merit of Gardner Murphy in this book that he is aware of this difficulty and attacks a problem that is prior to that of integration into a world perspective. This is the investigation of what does not at present seem to integrate. As the author says on page 1: "Psychical research, or parapsychology, consists of observations recorded in a form which aims at order and intelligibility, but which cannot by any stretch of the imagination be subsumed under the science of today. Shall we accept that which cannot be assimilated?" This book is directed towards the answering of this question and the answering of the question that arises from an affirmative answer to it: "What then at present seems to demand acceptance?"

In finding answers to these questions, Professor Gardner Murphy has abandoned the aim of giving an all-round account of the present position of psychical research. In this he has made his book different from the average text-book on the subject, and I should judge has made it more interesting and more profitable to one who is making his first acquaintance with the literature of the subject. Instead of a general view there are a number of highly particularized views of outstanding contributions to the subject. His method has been to select certain researches which illustrate different directions of psychical research and to present these in the original author's own words with the minimum of comment by Gardner Murphy himself.

A telescopic view of some of the outstanding peaks is a more imaginative and, for some purposes, a more effective way of becoming acquainted with a country than the study of a general map in less detail. Everything then depends on what choice of peaks is made; not all writers would make the same choice. The author's very wide knowledge of the literature of psychical research enables him to choose his peaks with sound judgment.

The first is a number of associated peaks in the region of spontaneous cases beginning with a case reported nearly eighty years ago in *Phantasms of the Living*. It concerns a Mr. Severn whose wife, lying in bed, had an experience as if she had had an injury to her mouth at the time at which her husband had actually received such an injury from the tiller of a boat in which he was sailing on a near-by lake. More recent cases include some reported to the A.S.P.R. and studied by Professor Murphy's collaborator in this volume, Mrs. L. A. Dale. There is also a selection of two out of four dreams reported by W. F. Prince in the *Journal of the A.S.P.R.* in 1923, and another dream suggesting determination by ESP which occurred in the course of psychoanalysis and is reported by Dr. Ehrenwald.

This selection of spontaneous cases introduces the range of non-experimental cognitive psi occurrences. As the author points out, the gathering of spontaneous cases never satisfies a scientist. A chapter on experimental telepathy gives a brief account of the early drawing experiments carried out by Warcollier and those of Whately Carington. A somewhat more detailed account is given of the experiments carried out at Groningen by Brugmans in the early

twenties. These were, no doubt, the best experiments of that time, carried out in the unusual location of an experimental psychology laboratory. Although Gardner Murphy thinks well of this experiment he asks some critical questions which are no longer answerable.

The section on experimental clairvoyance starts with the very successful Pearce-Pratt series of card-guessing experiments at a distance. He truly points out that these could only be given a normal explanation by making the very improbable suggestion of collusive fraud. This, however, I think, must be true of any experiment, however great the number of witnesses. Collusion involving many people is more improbable than collusion involving few but the probability does not become zero. Multiplication of experiments seems to be the way to meet this difficulty rather than elaboration of precautions against fraud within a single experiment.

Schmeidler's sheep-goat experiment is also discussed. Although its relatively low order of significance makes it less important than the Pearce-Pratt experiment as evidence for ESP, it has its own very great value as the first experimental verification of the commonly held opinion that the results of ESP experiments are considerably determined by the attitude of the people taking part and that they are more likely to be successful if subjects accept the possibility of ESP.

Finally there are the experiments on ESP with teachers as agents and their pupils as subjects which were started by van Busschbach in Dutch schools and were carried on by Anderson and White in American schools. These have been successful both in supplying evidence for the reality of ESP in this situation and also in demonstrating the dependence of ESP performance on such factors as attitudes of pupils and teachers towards each other. It is also an experiment which shows good promise of at least some measure of repeatability.

The following chapter on precognition deals with the Soal-Goldney experiment with Shackleton. Although this is undoubtedly one of the outstanding experiments in ESP, it is of doubtful standing as evidence for precognition, except for the relatively short series in which Shackleton guessed targets determined by counters Mrs. Goldney drew from a bag. Shackleton's other successes could be due to contemporaneous clairvoyance. Other experimental designs

more satisfactory as means of providing evidence for precognition might have been described although, so far, such designs have not yielded successes of anything approaching the level of the Soal-Goldney experiment.

Professor Murphy's courage does not fail when he faces the problem of PK, which is undoubtedly a much more difficult subject to present to the non-specialist reader. Two main experiments are presented in some detail. These are the 1945 experiment by Mrs. Dale at the A.S.P.R.; the other, the work done by Forwald in collaboration with Pratt during Forwald's visit to the Duke University Laboratory in 1957. By 1945 some of the crudities of early PK experimentation had been outgrown. Subjects no longer decided their own targets and it was ensured that every target was represented an equal number of times. The significance of the deviation from expectation in the A.S.P.R. experiment was not very high but was sufficient to be useful supplementary evidence for the reality of the effect reported by Rhine. Forwald's results at Duke are also an important landmark in the investigation of PK although they are only a small sample of the very striking results obtained by this assiduous and very successful worker.

There are a few minor blemishes in this chapter which will perhaps be corrected in a later edition. In a footnote on p. 165 there is an estimate of significance of a correlation coefficient made *after* a correction had been applied which changed its observed value of .30 to a corrected value of .46. The error is not, of course, Professor Murphy's but perhaps such an erroneous estimate should not appear without comment in a book which has his deservedly high authority. On p. 164 there is a curious passage in a discussion of the effect of dice bias on expectation of score in a PK test where it is said that it is impossible "for dice loaded to favor, say, the three higher faces not *to disfavor to approximately the same degree* the three lower faces." Psychical researchers have long learned the necessity for cautious statement but surely the word "approximately" in this passage is carrying caution too far.

In the penultimate chapter on survival the author goes largely to the English S.P.R. for his source material. As an example of the Cross-Correspondence type of evidence, he goes with considerable detail into the Lethe sittings and those connected with the Ear

of Dionysius. No doubt the cross-correspondences will be long discussed. If this was really an experiment engineered from the other side, it seems to me to suffer from the defects of over-ingeniousness and difficulty of assessment. I should have supposed that more direct evidence of communication would have been a better introduction to the difficulties of the survival problem. On the solution to that problem Gardner Murphy sits firmly on the fence. "To me," he says, "the evidence cannot be by-passed, nor on the other hand can conviction be achieved." There will be general agreement with his further contention that we need far more evidence.

Finally, the author devotes a relatively short chapter to "An Interpretation." What he says is judicious and penetrating. His interpretation is one that I think would be widely accepted among students of the subject. It is essentially that the potential importance of psychical research lies in the unexpectedness of its phenomena and the possibility that something can be demonstrated to occur that transcends the ordinary known relations of the organism to time, space, matter, and energy, which may require genuinely new principles to give a rounded interpretation. Its importance may lie in the part it plays in a shift to a new way of looking at life and mind.

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A Critical Examination of the Belief in a Life After Death by C. J. Ducasse. Charles C Thomas: Springfield, Ill., 1961. xvii + 318 pp. $8.75.

This extremely interesting book is divided, like Caesar's Gaul, into three parts. In the first part the problem is approached from a metaphysical standpoint. The second part is devoted to a survey of the evidence provided by the records of spontaneous occurrences and by psychical research, and to an evaluation of the same. The third part is concerned with the question of reincarnation, its philosophic tenability, and the kind of evidence which might conceivably substantiate it. To the mere philosopher the first is of especial interest. From the standpoint adopted by Professor Ducasse his discussion—as might have been anticipated—is on the whole ex-

tremely fair, and will, I think, impress the impartial reader as such. And I consider that he has made out his case that the hypothesis of survival is one which the philosopher ought to take very seriously.

There are, however, three relatively minor points with respect to which I feel impelled to register dissent. Professor Ducasse is quite convinced that the average man is not at all concerned with the possibility of everlasting life, but only with that of surviving death for an indefinite period. It is hazardous to pronounce as to the outlook of the average man, but, if the present reviewer can claim to rank as such, he is justified in entering a *caveat*. If I knew that I would survive death for a period of a million years only to be annihilated at the end thereof, I should, I am quite convinced, feel much greater regret than if I knew that I would not survive death at all. I am of the mind of the skipper in the *Bab Ballad* entitled *Brave Alum Bey* who delivered the following advice to the crew of his sinking ship:

> "To hooloa and kick is a very bad plan:
> You'd best get it over as soon as you can;
> You'd better get hold of a good lump of lead,
> And collar it tightly until you are dead."

It is said that the Chinese authorities of the imperial era used to have condemned pirates conducted every day to the execution grounds where swords were swished over their heads, with the ultimate result that the pirates broke down, wept, and begged for immediate decapitation. If Professor Ducasse be right, all humanity may stand in much the same relation to the universe as the Chinese pirates stood to their executioners. I seriously question whether the average man believes this; and I suspect that, like Sir Thomas Browne, he is "ready to be anything in the ecstasy of being ever."

Professor Ducasse is also clearly of the opinion that no *a priori* proof of immortality will stand. I, for one, do not concur in this verdict, but this is no place to argue the question. It is important, however, to note the fact; because it is for this reason that in the first part of his book he limits himself to trying to show that from the point of view of metaphysics survival remains an open question and that, in the second part, he rests his case wholly upon empirical evidence.

Again Professor Ducasse evidently believes that materialism and Berkeleyan idealism are equally indefensible, and open to the same sort of refutation. It does not seem to me that this is the case. To everyone except the extreme realist the existence of matter is a hypothesis, and one so difficult to establish that appeals are perpetually made to "animal faith," "intuition," or something of the kind, whereas of the existence of the self and of the reality of its experience we have immediate certitude—at least no one can deny the existence of mental states without contradicting himself. Hence, although not subscribing to Berkeleyan idealism, I would urge its higher claim.

As to the evidence adduced in the second part of the book, only an expert in the field of parapsychology is qualified to give an opinion. To the layman spontaneous occurrences are apt to seem far more impressive and interesting than those evoked under controlled conditions, but the disadvantage is that we have to rely so heavily upon the veracity and accuracy of verbal testimony. I find it difficult to believe that all the witnesses are either fools or liars, and there must surely be limits to the capacity for self-deception. But a little well authenticated evidence is preferable to a vast deal that is doubtful; hence the importance of the laboratory and its controls.

Professor Ducasse objects to Dr. Rhine's description of parapsychological phenomena as "non-physical" on the ground that it has not been proved that distance has no effect whatever upon their occurrence. Upon the justice of this criticism I am in no position to pronounce. Professor Ducasse is also prepared to deny the philosophical implications which, he tells us, Dr. Rhine proposes to draw from these phenomena; but here his criticism is too condensed to make discussion feasible.

An important issue, however, emerges from a perusal of Chapters XVIII and XIX. If we can take as established, as I presume we have every right to do, the occurrence of ESP in the forms of clairvoyance, clairaudience, precognition, and telepathy, it is natural and commendable that parapsychologists should seek to account in terms of these for all the phenomena in question without resorting to the hypothesis of survival. Professor Ducasse urges that this is to credit the subconscious mind with the capacity to make contact with all relevant material regardless of the limitations of time and

space, and thus practically to endow it with omniscience. This, he considers, is so to expand the notion of ESP as to subject it to an intolerable strain. Doubtless it may be unwise and presumptuous to attempt to tell parapsychologists their business. At the same time I confess that I am not without sympathy with Professor Ducasse's apprehension that a useful hypothesis is being ridden to death. But, if it require to be supplemented, we seem driven to posit survival.

On the other hand, if we concede that the range of ESP is practically unlimited, can we reasonably suppose that the self, which is so independent of its body as regards its activity, can be dependent upon it for its existence? That we cannot has been forefully contended by Professor H. H. Price in an article in the *Hibbert Journal*.[1] Thus it would appear that, whether his hypothesis be initially affirmed or denied, the believer in survival wins either way.

The remarks which I have made with respect to the discussion of evidence in the second part of the book apply also to that in the third. Much of the evidence is very impressive, on the assumption that the witnesses are competent and veracious. But here we have to consider, not one, but two additional hypotheses—survival and reincarnation. Despite Professor Ducasse's persuasiveness, I still feel uncertain whether the question of reincarnation could ever be finally settled by any conceivable empirical evidence. But, then, I am not an empiricist. And I should be the last to discourage an empirical inquiry with regard to an issue so vital to human happiness, an inquiry which may accomplish far more than we can at present imagine. Professor Ducasse's book should do much to stimulate minds that are not already closed. He has started a hare which will surely tax the united efforts of many hunters to run down.

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Extrasensory Perception. Edited by Fabian Gudas. New York: Charles Scribner's Sons, 1961. xviii + 141 pp. $1.95.

This useful and stimulating book is the fourth volume of Scribner Research Anthologies: paperback compilations of essays and ex-

[1] Vol. XLVII No. 2.

cerpts concerned with a single complex topic. The first three volumes deal with Shakespeare's Julius Caesar, the Hungarian revolt, and two modern American plays; it is gratifying but unexpected to find ESP in this company.

Most of the contributors are distinguished, and all are competent. Their concepts and language are at a professional level and can be read with profit by us and our colleagues. From editorial comments, however, it seems that the book is intended for high school as well as for college use; and the intelligent high school student should be able to read and comprehend most or all of it. The anthology permits such a student, without straining his library facilities, to take notes on the diverse thinking of scholars of different periods and different viewpoints. By proper selection he can choose adequate material for a paper on any of several subtopics, or he may use the book as a springboard for library research or his own thinking.

The opening and closing sections contain instructions for taking notes and for writing a paper. The body of the book consists of 25 selections: three from the seventeenth century, seven from the nineteenth and fifteen from the period between 1935 and 1955. They are printed in a rough chronological sequence. Logical as this order is, it seems a defect. The young student who conscientiously begins at the beginning and tries to read straight through will spend considerable time on Increase Mather's discussion of deviltry, Thomas Huxley's analysis of whether animals are automata, and the like, before he comes to the relatively modern work of Joseph Jastrow (1900). The resulting perspective can be an odd one, and the editor mildly discourages it in the following way. The book opens with a two page Preface, a nine page Guide to Research and a three page Introduction. At the end of the introduction comes the quiet sentence: "The articles by James, Rhine and Soal constitute the core of the anthology and can profitably be read first." This sensible recommendation is inserted so unobtrusively at the end of so many pages of editorial and instructive comment that the eager student is likely to disregard it until after he has sampled the text. The three articles to which the student is referred for initial reading are the ninth, thirteenth (or twenty-second) and seventeenth. They are not set off in any way from the others, either in the text or in the Table of Contents.

It is interesting to try to judge the impact of the book on someone to whom it is an introduction to parapsychology. My guess is that, if the reader is patient and systematic enough to read it thoughtfully all the way through, the effect will be more than a parapsychologist has a right to hope. The articles from the seventeenth century, with their preconceptions and phraseology so different from ours, are likely to seem quaint and of only historical interest, not useful in forming a serious opinion about ESP. The nineteenth century articles open with one by Faraday and two by Thomas Huxley which derogate supernatural claims. Next comes a circular from a short-lived Cambridge society which appeals for detailed factual descriptions of purportedly supernatural events. This is followed by Mark Twain's delightful anecdotes of "Mental Telegraphy," and an 1897 survey by William James of the accomplishments of psychical research. Immediately after is a thoughtful twelve page essay by Jastrow, showing why he regards "the inclination towards a telepathic hypothesis as the result of a logical defective attitude." The score is four against the validity of ESP, two in favor and one neutral; and one of the affirmative votes may be discounted because it is cast by a literary, not a scholarly, man.

The article in which Jastrow dismisses psychical research is followed by Skinner's 1937 attack on the initial evidence presented by Rhine, then by a curious experiment of Leuba's directed at explaining away, as procedural artifact, the positive evidence from other experiments. Next come a 1940 review by Rhine and a 1942 review by Pope and Pratt, which present the fuller experimental case for ESP. The eleven remaining articles are written by men who accept the evidence for ESP and discuss its implications. The overall message which is conveyed to the systematic reader by such a choice of articles would seem to be that skepticism about the fact of ESP has been out of date since Rhine wrote in 1940.

Eleven of the excerpts were published in the two decades before the book was printed (though none is more recent than 1955). Three are by philosophers (Broad, Joad, Ducasse); one by a psychiatrist (Ehrenwald: "Telepathy in Macbeth?"); one by a psychologist (Boring) and another by a biophysicist (Rashevsky) who have done no ESP research. The others are by Pope and Pratt, Thouless, Soal, Soal and Bateman, and Rhine. Although the ESP experi-

ments from this group are classics, and although many of their articles contained reviews of research, not one of the articles is a description of an ESP experiment. Since the major message of the book seems to be that skepticism about ESP was common until experiments were systematically performed, but has diminished (or died down) since then, the omission of any ESP experiment is curious. The usual analogy is to playing Hamlet without the Prince.

A book should, of course, be evaluated in terms of the purposes for which it was written. The preface states two purposes for the series of which this book is a part: giving "the student access to important sources" and giving him "either all his sources for a controlled-research paper or some of them for a library-research paper." Although there may be minor quarrels with certain inclusions and with the omission of a well conducted ESP experiment, it is my impression that these purposes are admirably fulfilled. However, any instructor who uses this book would be well advised to call the student's attention to the key closing sentence of the introduction, or else to direct the student's initial reading rather than merely to turn him loose on the rough chronological series.

For a professional journal like this one, it is probably also proper to attempt to evaluate whether a parapsychologist would want this book in his personal collection. For loaning out to a friend who wants to be informed, it is adequate but inferior to some of the surveys that have recently been published. For loaning to a friend who wants to come back and argue with you, it is good—as are the surveys. For one's own reading it is agreeable because it permits one to renew acquaintance with many wise or ingenious or controversial articles; and it is stimulating because, for everyone except the most scholarly, there will be some unfamiliar but relevant articles which it is profitable to assimilate. I recommend it as an interesting addition to a parapsychological library.

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PARAPSYCHOLOGICAL ABSTRACTS

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[This section consists of abstracts of reports published in other journals and of unpublished reports filed at the Parapsychology Laboratory at Duke University. Readers may obtain unabridged copies (Thermo-Fax) of the unpublished reports by writing to the *Journal of Parapsychology,* College Station, Durham, N. C. The price is ten cents per page.]

BELOFF, J., AND EVANS, L. A radioactivity test of psychokinesis. *J. Soc. psych. Res.,* 1961, **41**, 41-46.

No significant departure from chance expectation was obtained in 900 trials in which subjects attempted to exert a psychokinetic effect on the rate of radioactivity: in this case, on the number of alpha particles emitted when a crystal of uranyl nitrate was placed inside the chamber of a scintillation counter which contained a crystal of activated sodium iodide as the sensitive element. Some other use of quantal phenomena is suggested for possible future experimentation, such as an effort to influence the direction of radiation; this would be less "physically impossible" than the effort to influence rate of radiation and might "bring us closer to the core of the elusive phenomenon of psychokinesis."

BLEKSLEY, A. E. H. The evidence for ESP and the "fraud" hypothesis. *Indian J. Parapsychol.,* 1960, **2**, 121-26.

The author, who is head of the Department of Applied Mathematics at the University of Witwatersrand, Johannesburg, is seriously concerned to find that when all other criticisms fail to stand up against the evidence for ESP, some recent critics have resorted to an attack on the good name of the experimenter by suggesting fraud. This charge, without having been proven "to the hilt" by the critics before being publicly made, he says, is a "fantastic approach to a scientific problem." Indeed, it appears in these cases as if the critic has formulated an opinion beforehand on *a priori* grounds about what can and cannot happen, in contrast to the parapsycholo-

gist who attempts to find out by the only acceptable method of science, that of experiment.

OTANI, S. Tomokichi Fukurai, the pioneer of psychical research in Japan. *Indian J. Parapsychol.*, 1960, **2**, 106-11.

Scientific psychical research in Japan got its initial start largely through the efforts of Dr. Fukurai, who began investigations of clairvoyance in 1910 and continued them until his death in 1952 despite considerable public and academic opposition. His efforts consisted mainly of experiments in which non-professional mediums attempted to identify concealed targets.

PARSONS, D. The Black Boxes of Mr. George de la Warr. *J. Soc. psych. Res.*, 1961, **41**, 12-31.

The so-called "Black Box" phenomena are reviewed and examined from the point of view of how the claims made by the inventor of the apparatus (for the unorthodox diagnosis or treatment of human or animal ailments) might strike "a methodical man." The several types of apparatus employed by de la Warr and his associates are briefly described, and a list is given of 32 widely varying claims taken from a book which de la Warr co-authored. The number of tests on which these claims are based is only occasionally given. The condensed reports of nine independent investigators are presented, and a recent court case arising out of the claims is briefly summarized. The de la Warrs—practitioners in psychic healing, dowsing, etc.—are regarded as intelligent and sincere persons, but their claims come far from meeting the standards necessary for a methodically minded person who would wish to penetrate behind possible artifacts rather than merely to build them up further.

RYZL, M. Experiments relating to the influence of hypnosis on ESP. *Indian J. Parapsychol.*, 1960, **2**, 115-20.

Exploratory efforts at developing ESP in more than 200 hypnotized subjects have been undertaken over the past twelve years in Prague, Czechoslovakia. In a typical experiment a hypnotized subject attempted to guess the hand position of a watch-face target placed out of his range of vision. A number of observations about the nature of the ESP observed are presented.

SALTER, W. H., HEYWOOD, R., AND GREEN, C. Report (1959) on enquiry into spontaneous cases. *Proc. Soc. psych. Res.*, 1960, 53, 83-194.

The major portion of this report is devoted to an analysis by Celia Green of the spontaneous cases obtained in response to an S.P.R. questionnaire survey initiated in 1956. The historical background to the present inquiry is provided by W. H. Salter, and the immediate background information describing the purpose, conduct, and difficulties encountered in the study is given by Rosalind Heywood.

Although a total of 1500 cases were received, only 300 were of sufficient detail to be included in the present analysis. The method of classification is described and illustrated in detail, and extensive tables and graphs are included to show various breakdowns of the data, such as the distribution of cases according to the percipient's age-group, sex, marital status, state of consciousness, sense of conviction, and attitude toward the parapsychical. Thirty actual cases, in some instances summarized, are reproduced in an appendix.

Suggestions arising out of this analysis include the impression that "a certain degree of emotional intensity was a predisposing factor in the production of phenomena," and also, the observation that a comparatively large number of cases was reported to have happened to relatively young people.

WALTER, G. "The Neurophysiological Aspects of Hallucinations and Illusory Experience." (Fourteenth Frederic W. H. Myers Memorial Lecture.) London: Soc. for Psychical Research, 1960.

Recent advances relating brain anatomy and function to mental activity or experience are discussed from the viewpoint of one whose stated working hypothesis is that all mental or psychic phenomena can be rigorously described in terms of physiological mechanisms. The human population is seen as a continuum of cerebral vulnerability to hallucinogenic or illusory experience, from the majority of normal people who have hallucinations only under great stress or privation to certain epileptic patients, at the other extreme, who frequently and monotonously experience elaborate and usually stereotyped illusory scenes. "Spontaneous" hallucinatory experiences are

considered adequately described as neurophysiological anomalies arising from brain activities similar to those contrived in the laboratory, which arise quite often by chance because of the vast neuronic complexity of the brain and its intricate metabolic economy. Although the question of the nature of telepathic communication "cannot be answered unequivocally," hallucinatory experiences of a telepathic nature might result from the statistical clustering of preferred or stereotyped illusions among a population.

GLOSSARY

A description of the basic experimental methods, of the findings, and of the statistical procedures for evaluating ESP and PK results may be found in *Parapsychology, Frontier Science of the Mind* by J. B. Rhine and J. G. Pratt (published by Charles C. Thomas, Springfield, Illinois, U.S.A., Blackwell Scientific Publications, Ltd., Oxford, England, and The Ryerson Press, Toronto, Canada).

AGENT: The "sender" in tests for telepathy, the person whose mental states are to be apprehended by the percipient. In GESP tests, the person who looks at the stimulus object.

BT (Basic Technique): The clairvoyance technique in which each card is laid aside by the experimenter as it is called by the subject. The check-up is made at the end of the run.

CALL: The ESP symbol selected by the subject in trying to identify a target.

CHANCE: The complex of undefined causal factors irrelevant to the purpose at hand.

Chance Expectation = Mean Chance Expectation: The most likely score if only chance variation is present.

Chance Average: Mean chance expectation expresed as an expected score, generally in terms of average per run.

CHI SQUARE: A sum of quantities each of which is a deviation squared divided by an expected value. Also a sum of the squares of CR's.

CLAIRVOYANCE: Extrasensory perception of objective events as distinguished from telepathic cognition of the mental activities of another person.

CR (Critical Ratio): A measure to determine whether or not the observed deviation is significantly greater than the expected random fluctuation about the average. The CR is obtained by dividing the observed deviation by the standard deviation. (The probability of a given CR may be obtained by consulting tables of the probability integral, such as Pearsons.)

CR_d (Critical Ratio of the Difference): The observed difference between the the average scores of two samples of data divided by the standard deviation of the difference.

DEVIATION: The amount an observed number of hits or an average score varies from the mean chance expectation or chance average. A deviation may be total (for a series of runs) or average (per run).

DISPLACEMENT: ESP responses to targets other than those for which the calls were intended.

Backward Displacement: ESP responses to targets preceding the intended targets. Displacement to the targets one, two, three, etc. places preceding the intended target are designated as (−1), (−2), (−3), etc.

Forward Displacement: ESP responses to targets coming later than the intended targets. Displacement to the targets one, two, three, etc. places after the intended target are designated as (+1), (+2), (+3), etc.

DT (Down Through): The clairvoyance technique in which the cards are called down through the pack before any are removed or checked.

ESP (Extrasensory Perception): Awareness of or response to an external event or influence not apprehended by sensory means.

ESP CARDS: Cards, each bearing one of the following five symbols: star, circle, square, cross and waves (three parallel wavy lines).

ESP PACK: Twenty-five ESP cards.

Closed Pack: An ESP pack composed of five each of the five symbols.

Open Pack: An ESP pack made up of the ESP symbols selected in random order.

EXPECTATION: See *Chance.*

EXTRACHANCE: Not due to chance alone.

FREE MATERIAL: The stimulus objects of experiments in which an unlimited or unspecified range of stimulus objects in employed (as contrasted with methods such as card-calling in which the subject knows that the stimulus object is one of a known range).

GESP (General Extrasensory Perception): A technique designed to test the occurrence of extrasensory perception, permitting either telepathy or clairvoyance or both to operate.

MCE (Mean Chance Expectation): see *Chance.*

P (Probability): A mathematical estimate of the expected relative frequency of a given event if chance alone were operative.

PARANORMAL: Parapsychical or parapsychological.

PARAPSYCHOLOGY: A division of psychology dealing with behavioral or personal effects which do not fall within the scope of physical principles.

PERCIPIENT: See *Subject.*

PK (Psychokinesis): The direct influence exerted on a physical system by a subject without any known intermediate physical energy or instrumentation.

PLACEMENT TEST: A PK technique in which the aim of the subject is to try to influence falling objects to come to rest in a designated part of the throwing surface.

PRECOGNITION: Cognition of a future event which could not be known through rational inference.

PREFERENTIAL MATCHING: A method of scoring responses to free material. A judge ranks the stimulus objects (usually pictures in sets of four) with respect to their similarity to, or association with, each response; and/or he ranks the responses with respect to their similarity to, or association with, each stimulus object.

PSI: A general term to identify personal factors or processes which are non-physical in nature. It approximates the popular use of the word "psychic" and the technical one, "parapsychical."

PSI-MISSING: Exercise of psi ability in a way that avoids the target the subject is attempting to hit.

PSI PHENOMENA: Occurrences which result from the operation of psi. They include the phenomena of both ESP (including precognition) and PK.

PSYCHICAL RESEARCH: Older term used for parapsychology.

QD (Quarter Distribution): The distribution of hits in the record page (or in a logical subdivision thereof, such as the set or the half-set) as found in the four equal quarters formed by dividing the selected unit horizontally and vertically.

RANDOM ORDER: An order of events which displays no trends or regularities that would allow any inference regarding one event from one or more of the others in the series.

RUN: A group of trials, usually the succesive calling of a deck of 25 ESP cards or symbols. In PK tests, 24 single die throws regardless of the number of dice thrown at the same time.

SCORE: The number of hits made in any given unit of trials.
Total Score: Total of scores divided by number of runs.
Average Score: Total score divided by number of runs.

SD (Standard Deviation): Usually the theoretical root means square of the deviations. It is obtained from the formula $\sqrt{npq}$ in which n is the number of single trials, p the probability of success per trial, and q the probability of failure.

SERIES: Several runs or experimental sessions that are grouped in accordance with a stated principle.

SESSION: A unit of an ESP or PK experiment comprising all the trials of one test occasion.

SET: A subdivision of the record page serving as a scoring unit for a consecutive group of trials, usually for the same target.

SIGNIFICANCE: A numerical result is significant when it equals or surpasses some criterion of degree of chance improbability. The criterion commonly used in parapsychology today is a probability value of .01 or less, or a deviation in either direction such that the CR is 2.58 or greater.

SINGLES TEST: A PK technique in which the aim of the subject is to try to influence dice to fall with a specified face up.

SPONTANEOUS PSI EXPERIENCE: Natural, unplanned, occurrence of an event or experience that seems to involve parapsychical ability.

STIMULUS OBJECT: The ESP card or drawing or other object, some identifying characteristic of which is to be apprehended by the percipient.

STM (Screened Touch Matching): An ESP card test in which the subject indicates in each trial (by pointing to one of five key positions) what he thinks the top card is in the inverted pack held by the experimenter behind a screen. The card is then laid opposite that position.

SUBJECT: The person who is experimented upon. In ESP tests, most commonly the percipient (though also the agent in GESP and telepathy). In PK tests, any individual whose task it is to influence the objects thrown.

TARGET: In ESP (including precognition) the objective or mental events the subject is attempting to respond to. In PK tests, the faces of the die which the subject attempts to bring up in the act of throwing. In placement tests, the designated part of the throwing surface.

Target Card: The card which the percipient is attempting to identify or otherwise indicate a knowledge of.

Target Pack: The pack of cards the order of which the subject is attempting to identify.

Target Face: The face on the die which the subject tries to turn up by PK.

TELEPATHY: Extrasensory perception of the mental activities of another person (as distinguished from clairvoyant cognition of objective events).

TRIAL: In ESP tests, a single attempt to identify a stimulus object. In PK tests, a single throw of the dice or other objects thrown.